



PHILOSOPHICAL  
TRANSACTIONS,  
GIVING SOME  
ACCOUNT  
OF THE  
Present Undertakings, Studies, and Labours,  
OF THE  
INGENIOUS,  
IN MANY  
CONSIDERABLE PARTS OF THE WORLD.

---

VOL. LXV. For the Year 1775.

PART I.

---

L O N D O N,

PRINTED BY W. BOWYER AND J. NICHOLS:  
FOR LOCKYER DAVIS, IN HOLBOURN,  
PRINTER TO THE ROYAL SOCIETY.

---

MDGCLXXV.





## A D V E R T I S E M E N T.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume. And this information was thought the more necessary, not only as it had been the common opinion, that they were published by the authority, and under the direction, of the Society itself; but also, because several authors, both at home and abroad, have in their writings called them the *Transactions of the Royal Society*. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending

to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers, as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

At a C O U N C I L, January 28, 1773.

Resolved, That after Volume LXII. the *Philosophical Transactions* be published twice in a year; the first publication to be of the months of November and December of the preceeding year, and January and February of the current year, as soon as may be after February, under the name of the "first part" of the volume: and the second publication to be of the remaining months unto the recess of the Society, as soon as may be after the recess, under the name of the "second part" of the volume.

---



---

# C O N T E N T S

T O

V O L. LXV. P A R T I.

- I. *E*XTRACT of a Letter from Dr. John Ingenhoufz, F. R. S. to Sir John Pringle, Bart. P. R. S. containing some Experiments on the Torpedo, made at Leghorn, January 1, 1773 (after having been informed of those by Mr. Walfh). Dated Saltzburg, March 27, 1773. page 1
- II. *An Account of Two Giants Causeways, or Groups of prismatic basaltine Columns, and other curious volcanic Concretions, in the Venetian State in Italy; with some Remarks on the Characters of these and other similar Bodies, and on the physical Geography of the Countries in which they are found. Addressed to Sir John Pringle, Bart. P. R. S. by John Strange, Esq. F. R. S.* p. 5
- III. *An Inquiry to show, what was the ancient English Weight and Measure according to the Laws or Statutes, prior to the Reign of Henry the Seventh. By Henry Norris, Esquire. Communicated by Dr. Brocklesby.* p. 48
- IV. *The*

- IV. *The Description of an Apparatus for impregnating Water with fixed Air; and of the Manner of conducting that Process.* By John Mervin Nooth, M. D. F. R. S. p. 59
- V. *Account of a Musical Instrument, which was brought by Captain Fourncaux from the Isle of Amsterdam in the South Seas to London in the Year 1774, and given to the Royal Society.* By Joshua Steele, Esquire, in a Letter to Sir John Pringle, Bart. P. R. S. p. 67
- VI. *Remarks on a larger System of Reed Pipes from the Isle of Amsterdam, with some Observations on the Nose-Flute of Otaheite.* By Joshua Steele, Esq. p. 73
- VII. *Description of a new Dipping-needle.* By Mr. J. Lorimer, of Pensacola, in a Letter to Sir John Pringle, Bart. P. R. S. p. 79
- VIII. *Bill of Mortality for Chester for the Year 1773.* By J. Haygarth, M. D. F. R. S. p. 85
- IX. *Experiments on a new Colouring Substance from the Island of Amsterdam in the South Sea.* Made by Mr. Peter Woulfe, F. R. S. at the Desire of Sir John Pringle, Bart. P. R. S. p. 91
- X. *Experiments and Observations on the Gymnotus Electricus, or Electrical Eel.* By Hugh Williamson, M. D. Communicated by John Walsh, Esq. F. R. S. p. 94
- XI. *An Account of the Gymnotus Electricus, or Electrical Eel.* In a Letter from Alexander Garden, M. D. F. R. S. to John Ellis, Esq. F. R. S. p. 102
- XII. *Experiments and Observations in an heated Room.* By Charles Blagden, M. D. F. R. S. p. 111

- XIII. *The supposed Effect of boiling upon Water, in disposing it to freeze more readily, ascertained by Experiments.* By Joseph Black, M. D. Professor of Chemistry at Edinburgh, in a Letter to Sir John Pringle, Bart. P. R. S. p. 124
- XIV. *Experiments on the Dipping Needle, made by Desire of the Royal Society.* By Thomas Hutchins. p. 127
- XV. *A Meteorological Journal for the Year 1774, kept at the Royal Society's House by Order of the President and Council.* p. 139



---

---

PHILOSOPHICAL  
TRANSACTIONS.

---

- I. *Extract of a Letter from Dr. John Ingenhoufz, F.R.S. to Sir John Pringle, Bart. P. R. S. containing some Experiments on the Torpedo, made at Leghorn, January 1, 1773 (after having been informed of those by Mr. Walfh). Dated Saltzburg, March 27, 1773.*

Reddc, Nov. 10,  
1774. **A**S I could get no torpedos alive to my lodgings at Leghorn, I hired a fishing vessel, called a *tartana*, with eighteen men in her, and went out twenty miles to sea, where the bottom is muddy, and where those fish are chiefly to be found. We caught five; of which, four were about a foot in length, and the other of a smaller size. Before the nets were taken up, I charged a coated jar by a glass tube, and gave a shock to some of the sailors; who all told me, they felt

VOL. LXV. B the



the same sensation as when they touched the torpedo. Those people acquainted me, that this animal has but very little force in winter, and cannot live a long time out of the water. As soon as the whole quantity of fish caught was hauled up upon deck, I put the torpedos immediately into a tub, filled with sea water, together with two or three other fishes, which I found not at all hurt by their company. I took one of the torpedos in my hand, so that my thumbs pressed gently the upper side of those two soft bodies at the side of the head, called (perhaps very improperly) *musculi falcati* by REDI and LORENZINI, whilst my forefingers pressed the opposite side. About one or two minutes after, I felt a sudden trembling in my thumbs, which extended no further than my hands: this lasted about two or three seconds. After some seconds more, the same trembling was felt again. Sometimes it did not return in several minutes, and then came again, at very different intervals. Sometimes I felt the trembling both in my fingers and thumb. These tremors gave me the same sensation, as if a great number of very small electrical bottles were discharged through my hand very quickly one after the other. The fish occasioned the shock, or trembling, as well out of the water as in it. The shock lasted sometimes scarce a second; sometimes two or three seconds. Sometimes it was very weak; at other times so strong, that I was very near being obliged to quit my hold of the animal. The torpedo having given one shock, did not seem to lose the power of giving another of the same force soon again; for I observed

served several times, that the shocks, when they followed one another very fast, were stronger at last than in the beginning; and this was the same when the fish was under water as when kept out of it. The pressure of my fingers, more or less strong, did not seem to make any alteration in the powers of the torpedo. Applying a brass chain to the back of the fish, where I had put my thumb before, I found no sensation at all in my hand, though I repeated the experiment often, and applied the chain for a space of time in which I always perceived a stroke<sup>(a)</sup>. This was probably owing to the weakness of the fish in winter; or, perhaps, because I neglected to put my finger to its opposite side. Having insulated myself on an electrical stand, and keeping the torpedo in my hand, in the manner abovementioned, I gave not the least sign of being electrified, whether I received a stroke from the fish or not. The torpedo being suspended by a clean and dry silk ribbon, it attracted no light bodies, such as pith-balls, or others, put near it. A coated bottle applied to the fish, thus suspended, did not at all become charged. When the fish gave the shock in the dark, I heard no crackling noise, nor perceived any spark. When pinched with my nails, it did not give more or fewer strokes than when not pinched. But by folding his

(a) Dr. INGENHOUSZ means, that he felt no shock, though he saw the animal, by the contortion of its body, give one to the chain. At that time he did not seem to know, that though the shock would be communicated by a rod of any metal, it could not be so by a chain, or where there was the least interruption of continuity.

body, or bending his right side to his left side, I felt more frequent shocks. Dr. DRUMMOND made these experiments with me.

We dissected some of the torpedos, and found, if I remember well, four very large bundles of nerves, passing sidewards from the head into the two soft bodies, called *musculi falcati*, and distributed by dense ramifications through their whole substance. These nerves seem to terminate in round threads, which surround certain cylinders of a transparent gelatinous substance, which seems to constitute the material part of these singular bodies that appear to be the reservoirs of the electric power: these cylinders are parallel to each other, and have their direction from the under to the upper side of the fish. I did not observe whether these soft bodies changed in size when the torpedo gives a shock; but I suspect they do. I waited for a better opportunity to make a good many more experiments, and repeat, with more care, those already made. But though I have not been fortunate enough to find any other than what is above mentioned, I thought it my duty to inform you of what I have attempted, howsoever incomplete my researches have been. And if ever any further opportunity should offer, and in a better season, I shall not fail to make all the experiments I can think of, for illustrating so curious and interesting a subject. I am, &c.

II. *An Account of Two Giants Causeways, or Groups of prismatic basaltine Columns, and other curious vulcanic Concretions, in the Venetian State in Italy; with some Remarks on the Characters of these and other similar Bodies, and on the physical Geography of the Countries in which they are found. Addressed to Sir John Pringle, Bart. P. R. S. by John Strange, Esq. F. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

*Naturalem causam quærimus et assiduam, non raram et fortuitam.*

SEN. Nat. Quæst. L. ii. C. 55.

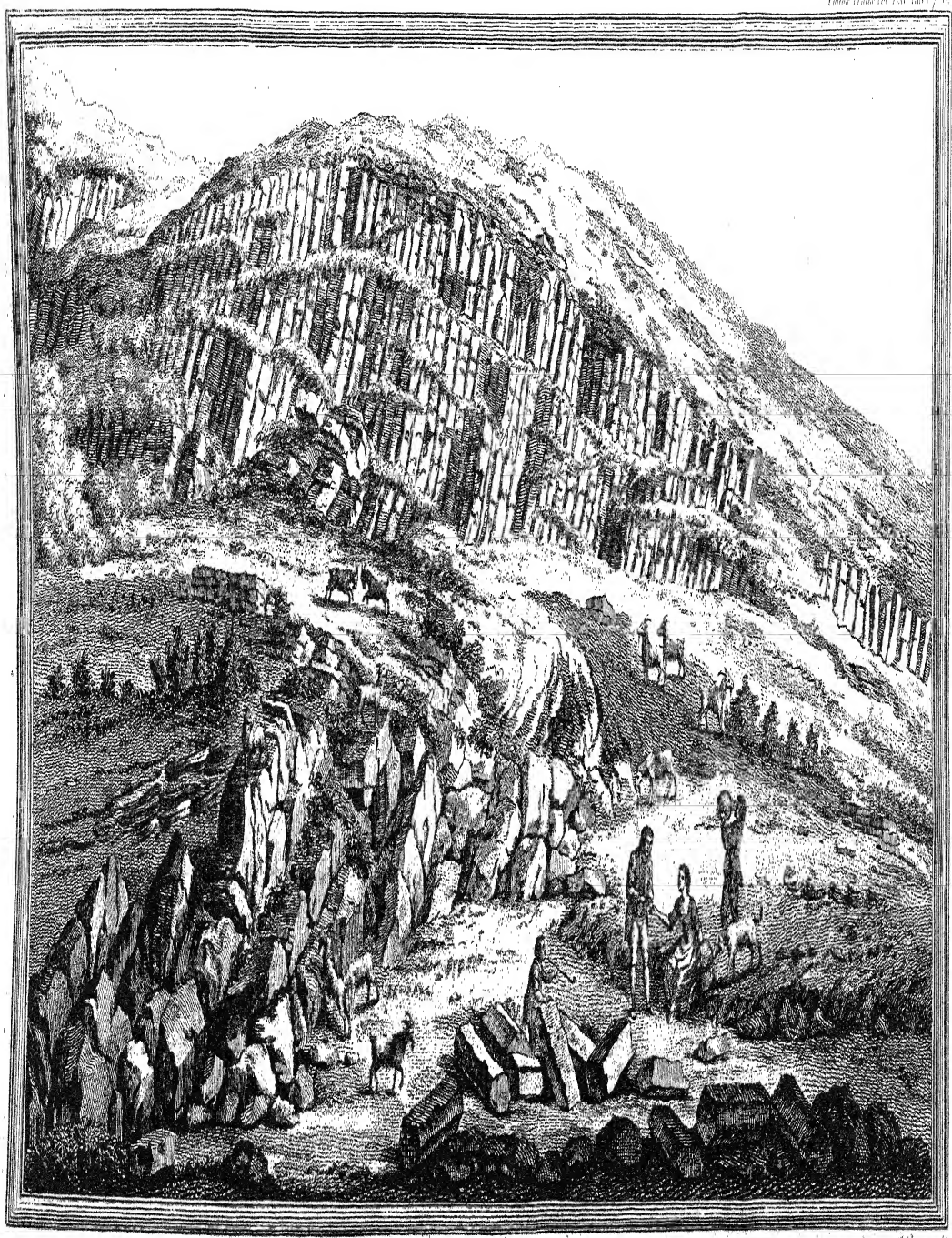
Redde, Nov. 24,  
1774.

HAVING had some satisfaction in the discovery of two groups of prismatic basaltine columns in the Venetian State in Italy, I thought, that an account of them might possibly prove acceptable to you, SIR, and to the other learned members of the Royal Society. I therefore take the liberty of transmitting to you the present, together with accurate drawings of these causeways, requesting of you to communicate them to the gentlemen of the society, should you think they merit their attention. I shall, first, briefly explain the

the two drawings; and then add such observations as have occurred to me, upon considering more particularly the curious originals which they represent.

N. 1. Is a topographical view of a part of the south-east side of a hill, called MONTE ROSSO, about seven miles distant, nearly south; from Padua, in the Venetian State in Italy, and a mile to the west of ABANO, a village well known, from the celebrated hot baths of that name, and which are situated at half a mile's distance to the south of it. This view particularly represents a natural range of prismatic columns, of different shapes and sizes, which are placed in a direction nearly perpendicular to the horizon, and parallel to each other, much resembling that part of the famous Giant's Causeway in Ireland, called THE ORGANS, as may be seen at Fig. 2. in the west prospect of that Causeway, engraved by Vivares, after one of Mrs. S. Drury's excellent designs. N. 2. Is a similar representation of the west side of another basaltine hill, called IL MONTE DEL DIAVOLO, or the DEVIL'S HILL, near San Giovanni Illarione, also in the Venetian state, and Veronese district, about ten miles nearly north-west of Vicenza. The prismatic columns appear to be ranged in an oblique position, along the side of the hill, not unlike the group represented under the rock, marked Fig. 9. in Mrs. DRURY's west prospect of the Giants Causeway. This drawing, however, represents only a part of the Causeway of San Giovanni, which continues along the side of a valley, nearly in the same manner, to a considerable distance.











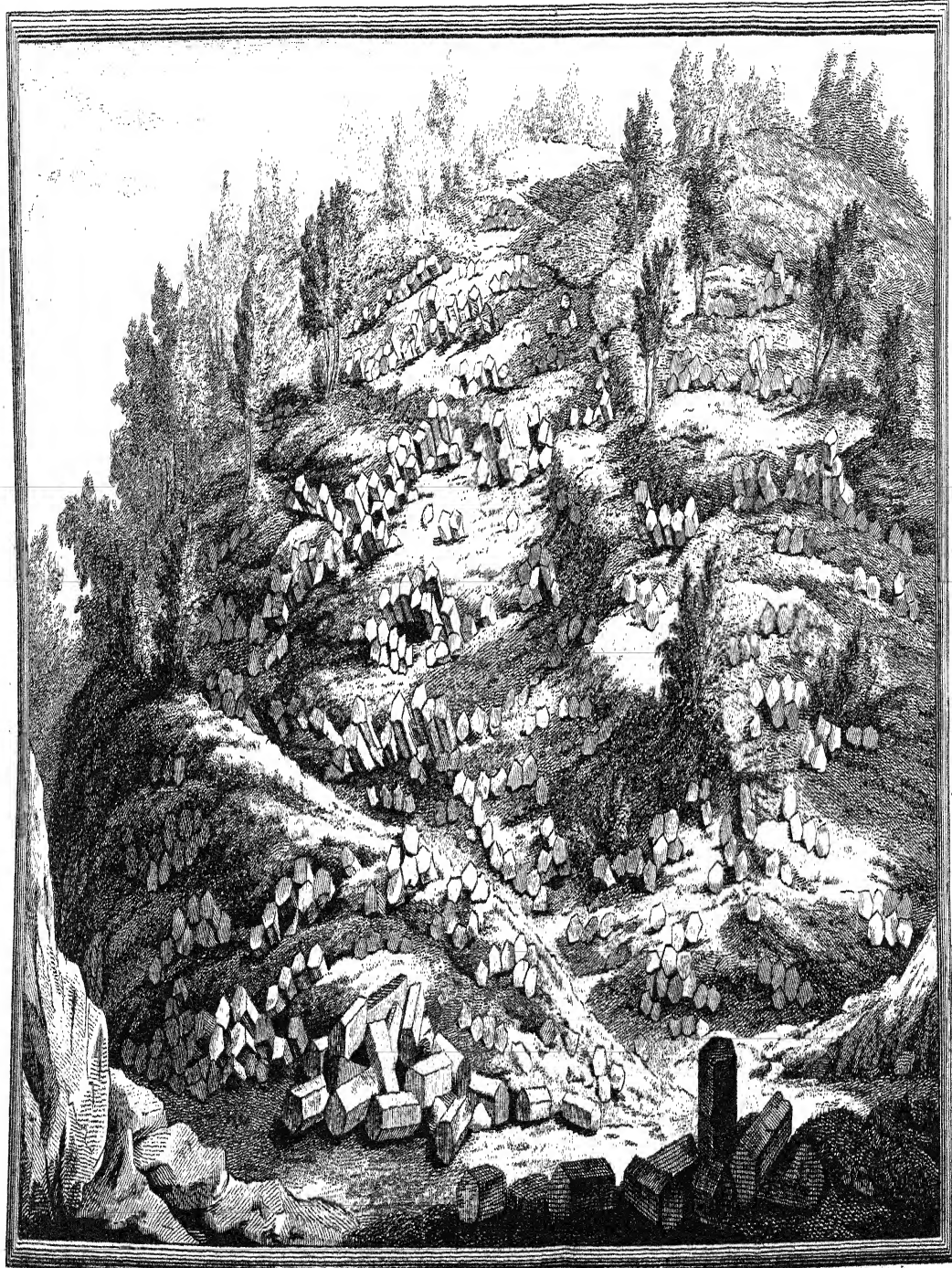
distance. Though the columns of both these hills are of the simple, or unjointed species, *Corneus crystallifatus prismaticus lateribus inordinatis*, WALLERII, yet they differ very remarkably from each other in many respects; but principally in their forms, and the texture and quality of their parts. Those of San Giovanni commonly approach a circular form, as nearly as their angles will permit; which is also observable in the columns of the Giants Causeway, and of most other basaltine groups. On the contrary, those of Monte Rosso rather affect an oblong or oval figure, as may be more particularly observed in the annexed representation of one of them<sup>(a)</sup>. The columns of San Giovanni measure, one with the other, near a foot in diameter; nor do they vary much in their size; though this is often the case in similar groups, and is particularly observable in that of Monte Rosso, whose columns sometimes equal nearly a foot in diameter, while others scarcely exceed three inches: the common width of them is about six or eight inches. They differ, therefore, very considerably in size from those of the Giants Causeway; some of which, as is well known, measure two feet in width. I can say nothing certain concerning the length of the columns of San Giovanni, since they present only their tops to view; the remaining parts of them being deeply buried in the hill, and in some places intirely covered, as may be seen in the drawing. The columns of Monte Rosso, as far as they are visible, measure only from six to eight or ten feet in height;

(a) Fig. 1.

which

which is also a small size, when compared with the height of those of the Giants Causeway, some of which measure near forty feet. The columns of the Venetian groups manifest, however, all the varieties of prismatic forms, that are observable in those of the Giants Causeway, and other similar groups. But they are commonly either of five, six, or seven sides; and the hexagonal form seems mostly to prevail, which, if I mistake not, is also remarkable in the Giants Causeway, and, as I believe, in most others. Nor is there less difference in the texture and qualities of these columns, than in their forms. Those of San Giovanni present a smooth surface, and, when broken, appear within of a dark iron grey colour, manifesting also a very solid and uniform texture; in which characters they correspond with the columns of the Giants Causeway, and those of most other basaltine groups. But the columns of Monte Rosso are very different in all these respects. For they have not only a very rough, and sometimes knotty surface; but, when broken, manifest a variegated colour and unequal texture of parts. I have broken several, and have constantly found them of this heterogeneous character, and conclude, that the rest are, as usual, of the same; nor do I apprehend, that, among the whole, there is a single column of an uniform colour and texture, like those of most other groups. They are commonly speckled, as it were, more or less distinctly, and resemble an inferior sort of granite, of which Monte Rosso itself is formed, and which serves as a base to the range of columns in question. It is, in general, not quite









so hard as the Alpine and Oriental granites, and is sometimes even friable, like the *Saxum granites particulis parum adhaerentibus*. ANON. Min. 270. n. 1.; or, *Saxum micaceum quartzosum spatiosumque subfriabile*. LINN. Syst. Nat. tom. iii. p. 76. edit. Holmiæ 1768. LINNÆUS justly observes, that this species of granite abounds in France; for I have lately seen large tracts of it in the neighbouring provinces of Auvergne, Velay, and Lionnois; and apprehend, that it likewise abounds in the Vivarey, Gevaudan, and Sevens mountains; from the affinity observable in the physical geography of those countries. But it is equally common in Italy; for besides Monte Rosso, the bulk of the Euganean hills in general, of which that is a part, principally consists of it; and these hills occupy a considerable tract in the plains of Lombardy. It is also common in the Tuscan and Roman States; the mountain close to Viterbo, on the road to Rome, is entirely composed of it. The Italians call it *Granitello*, and it much resembles the *Lapis variolatus* described by ALDROVANDUS in his *Museum Metallicum*. Though partial spots of this granite are often friable, especially about the surface, yet in general it is very hard; inso-much, that M. GUETTARD, compares the granites of France with those of Egypt<sup>(b)</sup>. The columns of Monte Rosso appear therefore of a different character from any hitherto described by mineralogists, who only mention those of an uniform colour and texture. It has been

(b) Mémoires de l'Acad. pour 1751.



observed, that the mass of stone in the hill above the Giants Causeway, portions of which are represented at Fig. 9. in Mrs. DRURY's west prospect, is of the same quality and texture with the columns themselves; which affinity I have constantly observed between other similar basaltine groups, and the masses to which they belong. It is not therefore extraordinary, that the same should also be observable, between the columns of Monte Rosso, and the sort of granite rock on which they stand, and with which they seem, in a manner, intimately connected. For it is further remarkable, that the masses, or strata, of this granite, though irregular, are yet ranged nearly in the same direction with the columns above them, as may be observed, if I mistake not, even in the drawing. But the great singularity here is, that such a range of prismatic columns should be found bedded, as it were, in a mass of granite, and composed nearly of the same substance; of which I never yet saw or heard any other instance. This circumstance seems therefore to render the causeway of Monte Rosso more curious and singular than the famous one in Ireland is known to be, from the regular articulation of its columns; the same phenomenon having lately been discovered at Staffa, one of the Western Islands of Scotland. Different groups of articulated basaltine columns have likewise been observed in the province of Auvergne in France; particularly by M. BEOST DE VARENNES<sup>(c)</sup>, at Blaud near Langeac; and

(c) Sage Elements de Mineralogie Docimastique, Paris, 1772, 8vo.

by M. DESMARESTS, near *le Mont d'Or*<sup>(d)</sup>. M. SAGE also mentions another near St. Alcon, in the same province. The Monte Rosso group is, however, not only curious in itself, but very interesting, on account of the great light it seems to throw upon the origin of granites in general, and I shall have occasion to observe more particularly hereafter. It is remarkable that the columns, in the two different groups of Monte Rosso and San Giovanni, preserve respectively the same position, nearly parallel one with the other; which is not commonly the case in other basaltine groups. For though the principal aggregate, which forms the Giants Causeway, stands in a direction perpendicular to the horizon; yet other small detached groups of columns also appear in the hill above, that affect, by their position, different degrees of obliquity. Among the numerous basaltine hills of Auvergne and Velay, in France, many of which I have lately visited, and which seem to abound in those provinces more than in any other part of Europe, and perhaps of the known globe, nothing is more common, than to see the columns of the same group lying in all possible directions, as irregularly almost, as the prisms in a mass of common crystal. Nor is this variety of position so observable in single columns, as in whole masses, or ranges of them, which often present themselves, in the same hill, disposed in different *strata*, or stages, as it were, one above the other, many of which affect very different, and even opposite, directions.

(d) De Romè Delisle Essai de Cristallographie. Paris, 8vo, 1772.

Thus, for instance, I have often seen a range of horizontal columns placed against another, in which all have appeared perpendicular; while a third mass, adherent to one or other of the former, and perhaps to both, has presented itself, with its columns obliquely disposed. It appears then, that a perpendicular position, with respect to the horizon, is by no means a characteristic peculiar to basaltine crystallizations, as hath been commonly pretended. On the contrary, whole groups frequently occur, that exclusively affect particular degrees of obliquity; as is evident in those of Monte Rosso, San Giovanni, and many others. Nor is even the horizontal position, though less common, to be excluded, as I have just observed; though I have never yet seen an entire group of columns so disposed. Such a group may, however, possibly be found, among the great variety of similar phænomena, which those curious and most interesting provinces of Auvergne and Velay present to our notice. The columns of San Giovanni seem bedded in a kind of volcanic sand, which, in many parts of the hill, intirely covers them; nor do I recollect whether any other solid masses appear, besides the columns: these, however, probably rest at bottom upon a base of basaltine rock of the same nature. Nothing is more common, in the provinces of France just mentioned, than to see isolated basaltine hills almost exclusively composed of different layers of columns, which present themselves in stages, one above the other, often without any other *stratum* between them, resembling, in some measure, *si magna licet componere*

*componere parvis*, a huge pile or stack of cleft wood. Though I do not mean to engross this Paper with my observations on the Auvergne and Velay basaltes, which I shall refer to a future occasion, yet I cannot quit the subject without adding a few particular remarks, that immediately concern the present inquiry. Though the columnar crystallization of Monte Rosso is the only one I have yet seen, or heard of, in a mass of granite; yet other groups of columns have occurred to me in other parts, that are equally of a heterogeneous substance or texture, though different from those of Monte Rosso, as well as from the common basaltes. But I shall mention only one instance, as the most pertinent to my present subject, in the basaltine hill, called *Les Rameaux*, near Menchaux in Velay. By their form, surface, internal colour and texture, the columns of this hill partake of the characters both of the common basaltine columns, and of those of Monte Rosso before described. They approach nearer to the sub-oval than the circular form; their surface is rough, though not knotty; and though they rather incline towards the dark colour, and hard uniform substance, as usual; yet, on breaking several, I found them unequal, both in colour and texture, and sometimes interspersed with irregular pieces or patches, as it were, of a heterogeneous hard substance, which, by its *micæ*, and small rhomboidal crystallizations, much resembles a sort of granite I have frequently seen. The mass, on which these columns stand, is of the same mixed character; and towards the base of the hill,

a granite.

a granite predominates of the same nature with that observed in the columns. It is also remarkable, that granite in general, throughout Velay and the neighbouring province of Auvergne, is frequently intermixed with the basaltine, and other common volcanic hills. I have observed the same in Italy, particularly in the Euganean hills near Padua, and on the confines of the Roman with the Tuscan State, about Viterbo, Bolfano, &c.; which tracts are also mostly volcanic. The mountain of Radiconfani, and its environs, with those of Aquapendente, are chiefly of the same character; and near the lake of Bolfano, by the road side leading to Viterbo, is the group of prismatic basaltine columns described by KIRCHER in his *Mundus Subterraneus*<sup>(e)</sup>, and which is the only one in Italy known to me, besides those of the Venetian State. But the profusion of basaltine phænomena in the provinces of Auvergne and Velay is really surprizing. In a morning's ride, of about a dozen or fifteen miles, round Isenchaux, which is the center of the Velay basaltes, I counted twelve distinct groups of columns, in so many different hills, detached, and at a distance from each other; and as these presented themselves to my view accidentally, without going out of the way in search of them, it is to be presumed, that many others, in the same neighbourhood, probably escaped me. They also abound about Puy, the capital of Velay, and still more so throughout all Auvergne. Nor is it merely a church, a castle, or perhaps a village, as in other countries, that sometimes crowns

(e) Lib. viii. sect. 1. c. 9. &c.

the basaltine hills of Auvergne and Velay. Whole cities are built upon them; a remarkable instance of which, among others, occurs in the episcopal city of St. Flour in Upper Auvergne, which covers the summit of a basaltine hill, and boasts a Giants Causeway for its foundation. This is more particularly seen at the south-east corner of the hill, above the bridge, and on the outside, under the wall of the town; which circumstances I mention, in case the same curiosity should ever lead any other traveller into those parts. As St. Flour is confined to the isolated summit of a hill, and is very closely built, the circumference of the walls scarcely exceeds much above a mile; but the same causeway continues from under the town; on the north side, to a considerable distance through the remaining lower part of the same hill, upon which the hospital of the town is built. Under this hospital to the west, and by the side of a road leading down into the valley from the town, the causeway is quite open to view, for a great extent, presenting the most considerable suit, or continued range, of high columns, that I saw throughout the whole tour. Including the continuation of this causeway, under the town, and the remaining part of the hill, it forms an aggregate of columns, which, for extent and importance, may almost be compared with the famous Giants Causeway in Ireland; with this signal advantage in favour of the Auvergne group, that it affords the foundation to a considerable city. The columns of St. Flour differ also from any I have yet seen. Among other singularities, which I shall not consider at present,

I observed,

I observed, that their shafts, though of the usual prismatic form, are nevertheless sometimes wreathed, or twisted, like the artificial, round, and spiral columns, that are often, though barbarously, introduced in buildings. The substance of these columns is, however, of the common sort, like that of the columns of San Giovanni, and the Giants Causeway. The town of Chillac, a few miles above old Brioude, on the river Allier, in Upper Auvergne, is also built on a Giants Causeway, consisting of high, strait, but unjointed columns, which are open to view towards the river. But I shall give no further instances of this kind at present; only must beg leave to observe, before I quit the subject of prismatic columns, that although no group of them has hitherto been discovered in our island, yet I am persuaded, that the mountains of Wales contain one; having, in my tour of that country, observed several large pieces of such columns at Townen, on the sea coast of Merionethshire, not far from Dolgelthy: particularly about Townen church-yard, where they are used as posts. I could not learn from whence they came, but should suspect, from the character of the adjacent country, that they are found in the mountains towards Dolgelthy, and probably somewhere about the famous Cader Idris. For I afterwards observed, in my ascent of that mountain from Dolgelthy, that its predominant mass appeared to be a sort of vitrifiable stone, seemingly of igneous origin, and which I shall here take occasion to observe greatly prevails in North Wales, particularly in the three Alpine Counties of Merioneth, Montgomery, and Carnarvon.

Having

Having dwelt a little upon the subject of basaltine organization, I shall beg leave to add a few remarks concerning the origin of these bodies. The systematical mineralogists, in general, assign the same common origin to most lapideous solids, which they suppose to be generated by deposition from an aqueous fluid. In whatever manner, therefore, the prismatic bodies in question are classed upon such a principle, no adequate idea can thence be ascertained concerning their origin, which seems manifestly different. For surely the structure and other phænomena of these bodies sufficiently prove them to be crystallizations or concretions of a particular kind, and generated immediately from an igneous fluid: for they are not only peculiar to volcanic tracts of country; but differ, in every respect, from common crystals produced from an aqueous fluid. Every one knows, that the latter are formed *stratum super stratum*, by a slow and successive deposition and *juxta-position* of parts, as hath been proved satisfactorily by CAPPELER, LINNÆUS, and other writers on this subject. The same mode of generation is more particularly explained by STENO, in his excellent treatise *De solido intra solidum naturaliter contento*. But this mode does not seem at all reconcileable with the basaltine crystallizations in question. For however these bodies may vary in their texture, none of them, notwithstanding, afford the least indication of an origin common to other crystals; but seem rather the effects of some intrinsic principle of organization, by which they appear to have been produced simultaneously,



taneously, in a manner, upon the consolidation of the whole mass of matter, in which they lie, and with which they constantly bear the greatest analogy, as I have before observed. It is further remarkable, that common crystals are parasitical bodies; whereas basaltine crystallizations, notwithstanding the peculiarities of their figures, rather seem to form integral parts of the masses to which they adhere; and seem to acknowledge, with them, one common and simultaneous origin; like the rhomboidal and other crystallizations in granites, and other similar vitrifiable compound stones. Nor does the common flow and limited principle of crystallization seem at all adequate to so great an effect, which seems exclusively attributable to an igneous fluid, on the general concretion of which, the organic principle may be supposed to have operated simultaneously in a large mass, and produced these bodies in the same manner as a linget of metal concretes at once in the mould. No other mode of generation seems reconcileable with the phenomena of basaltine aggregates, as I shall more fully prove in the account of those of Auvergne and Velay. Nor do I pretend to determine how strictly this simultaneous concretion of parts may be applied to the organization of such bodies, having used that expression rather in opposition to the slow and interrupted succession, that, to all appearance, takes place in the formation of common crystals. It seems also further evident to me from the phenomena, that prismatic basaltine crystallizations, and other regularly figured volcanic groups (for others have

have lately occurred to me in Auvergne and Velay, which have never yet been considered, and of which I shall hereafter give an account) it seems, I say, evident to me, that such regular bodies have been generated locally, and not in the midst of those violent convulsions of Nature, which are commonly assigned for the origin of volcanic mountains in general. That the principle of organization, whatever it be, operates locally, in the formation of these bodies, appears, I think, sufficiently evident from the regular disposition and other particular characters of their groups. For notwithstanding the various directions of the columns, and masses composed of them, in the different groups, as I have before observed; yet, in other respects, the greatest regularity of disposition is commonly observable. They form *strata*, which are uniformly organized, disposed in particular directions, and often constant in the same to a great extent. These *strata* not only manifest a parallelism between their regularly figured parts, but in their whole aggregates; which often form extensive horizontal beds, and of an equal thickness throughout. This parallelism is also equally remarkable in groups that are composed of many *strata*; as I have particularly observed in those of Murat, and the Castle Hill of Achon, in Upper Auvergne; in which the columnar *strata* are not only parallel in themselves, but preserve, in their position, a parallelism with the other *strata* of the respective groups, which lie in regular stages, one above the other: and since these groups commonly form, in a manner, integral parts of the masses, or mountains,

tains, in which they are found, and these manifest also some affinity in their structure; it seems most reasonable to assign to both one common origin. The received notions, concerning the mode of origin of volcanic hills, do not therefore seem intirely to correspond with the phænomena of Nature; since naturalists generally suppose, that they have all been thrown up, from the bowels of the earth, by subterraneous explosions, like the Monte di Cenere, near Puzzuoli, in the kingdom of Naples, the island of Santorino in the Archipelago, or the ejected entrails of Vefuvius and Ætna. But surely this must appear a mistaken notion to any one who compares these tumultuary and inordinate aggregates, with the regular volcanic organizations before described. For in fact, what does Vefuvius, Ætna, the Monte di Cenere, and such like erupted piles, present to us but a heap of ruins, which evidently manifest the casual and extraordinary cause, to which they avowedly owe their origin? But this origin seems irreconcilable with the regular structures before mentioned, as may perhaps satisfactorily appear, from my considerations on the particular phænomena, that characterize them. And though it is very possible, that such organizations may sometimes take place upon the concretion of liquified matter thrown up in volcanic eruptions; yet, however similar they may be, from the nature of their origin, I can hardly imagine they can form other than imperfect and irregular masses. For however wonderful the rivers of *lava* of Vefuvius or Ætna may appear to us; they, in reality, are but partial and

4

tumultuary

tumultuary efforts of nature, that by no means seem adequate to the production of a Giants Causeway, or the basaltine organizations of Auvergne and Velay, several of which continue, almost uninterruptedly, for many miles. This I have more particularly observed in the solitary and horizontal basaltine *strata* that cap the high, though flat, volcanic hills in Auvergne, and the adjacent part of Velay; as may be more particularly seen at the brows of those hills in the respective vallies. The celebrated M. DE BUFFON, speaking of these hills, styles them *Des plaines en montagnes, qui forment des pays au dessus des autres pays* (*f*). They also prove the mistake of some naturalists, who falsely ascribing to volcanic hills in general the same origin, as to Vesuvius and Ætna, exclusively assign to them also the same conical or orbicular forms. But, if I mistake not, the particular and relative characters of basaltine, and other regular volcanic organizations of the like kind, contradict a casual and tumultuary cause, and evince the necessity of their local origin upon a more steady and uniform principle. I shall also further observe, that I never saw any certain vestige of a regular *crater* in any spot characterized by similar organizations; which, as I before remarked, form, in general, integral parts of the masses to which they adhere, and which also frequently manifest an analogous structure, however irregular. And so far are they from representing the ruinous scenes of Vesuvius or Ætna, that they often afford no loose or isolated masses, except fragments of the columns

(*f*) Histoire Naturelle, tom. ii. p. 11.

of such groups, which have given way through time. This I have more particularly observed in the vulcanic and columnar hills of Isenchaux in Velay; in which the groups of columns are often so united with the body of *lava*, that they form, in a manner, but one solid, though figured mass. Monte Rosso is precisely of this character; nor did I observe a single column, or fragment even, loose; those in the drawing being merely introduced to show the forms of the columns. Neither are there here, or in the basaltine hills near Isenchaux, any signs of a *crater*: on the contrary, these hills, as well as Monte Rosso, are mostly terminated by regular convex summits, that form a solid mass. And that fused masses should frequently concrete in such a form we need not wonder, if we reflect on the effervescent and expansive property of fire. The phenomenon of horizontal vulcanic hills is accountable upon another principle, and seems chiefly to depend on the state of those hills before their ignition; as I shall endeavour to prove in my account of those of Auvergne; and of which the vulcanic hills of the Veronese and Vicentine districts afford also singular instances, which I shall now consider. It is difficult to say in what state vulcanic hills of a particular and regular structure, like the basaltine hills, for instance, may have pre-existed, before their alterations by fire, since they afford evident proofs, not only of a liquefaction, but of an entire new organization; by which means all marks of their former characters are totally effaced. Notwithstanding which, since these organizations are generated locally,

some

some light is often to be had, even in this difficult question, and merely from the physical geography of the country, independent of the particular structure of the hills themselves. For countries have their external characters, according to the nature of the hills that compose them; though these characters seldom form a part of the geographer's inquiries; geography having never yet been formally considered, but as subservient to civil history. I shall not enter particularly into this question at present; only shall observe, that the difference, in the external characters of mountains, according to their internal structure, is easily seen, on comparing, for instance, the outward forms of mountains of granite, or other similar-vitrifiable compound stones, which are of an irregular structure, with those of limestone, which are commonly formed in regular *strata*. Signal examples of this are observable in the chain of mount Jura, which is exclusively calcareous, and of a horizontal summit, and that of the Alps, whose highest mountains are mostly of granite, and terminate in pics, pyramids, and other irregularly pointed forms, according to the nature of such mountains. When therefore a remarkable similitude is observable, between the forms and disposition of the hills and vallies of a volcanic district, and those of other countries of a certain character, that have not suffered by fire, it is reasonable enough, upon the principles before adopted, to suppose the same similitude even in the primary structure and qualities of the former, however they may have been obliterated by the intervention of fire. The volcanic

vulcanic districts of Auvergne and Velay, as well as those of the Venetian state, afford proofs enough of the truth of this opinion; but I shall confine myself at present to the latter, and particularly to the phenomena of this kind, which I observed in the Vicentine and Veronese mountains, and which, if I mistake not, will appear decisive in the question before us.

These mountains occupy the lower skirts of the Alps, on the north side of Lombardy, and are partly vulcanic, and partly of limestone. They form sub-divisions, or lateral branches of the great chain of the Alps, from which they diverge, nearly at right angles, and extend in a southern direction, and parallel with each other, towards the plain. Some of these branches are intirely of limestone, without any *lava*; others are composed of a mixture of both; and others again are exclusively vulcanic. I have rode from the point of one of them near Montebello, in the Veronese territory, to Bolca, always upon *lava*, for the distance of near twenty miles. It is along this branch, by the side of the valley leading to Bolca, and about four or five miles short of it, that the causeway of San Giovanni Illarione before described is situated. The whole solid mass of this branch, as far as I could observe, is almost intirely composed of *lava*, which, about the skirts and surface particularly, is of various kinds. Among others very curious, I remarked some, at the foot of the castle hill near Montebello, concreted in different masses, which, by their extreme hardness, heterogeneous texture and colour, very much resemble an ordinary sort of porphyry. But I saw

no granite mixed with the *lava*, in these lower limestone skirts of the Alps, though it abounds so much in the neighbouring Euganean hills, as well as in Auvergne and Velay, as I before observed. Notwithstanding the general volcanic character of this branch of Montebello, its original structure and characters are still very evident, and perfectly correspond with those of the neighbouring branches, that have never suffered fire. For though some new igneous modifications have accidentally and partially taken place about the skirts and surface of this branch; yet in other more internal parts, not only the original horizontal position and parallelism of the *strata* are manifest, but small, though integral parts of those *strata*, here and there, remain unburnt, and show their calcareous qualities, structure, and extraneous contents, perfectly similar to those of the other neighbouring mountains, that have never suffered by fire. This I particularly observed along the brows, or upper lateral *strata* of the volcanic branch just mentioned, above the valley between Sorio and Montebello. The famous fossil fish quarry at Bolca, so well known to all the curious in Europe, is only an unburnt, calcareous, and flaty point, or side promontory, as it were, of the highest part of the same volcanic branch, that descends into the valley, from the church and village of Bolca, which are built upon it. This point within unites immediately with the *lava*, forming, in a manner, an integral part of the same hill. In other parts again, and particularly at Ronca, also in the Veronese territory, a few miles to the north-west of



Montebello, though the mass is converted intirely to *lava*, and has evidently concreted from a fusion, yet the marine fossil bodies, originally contained in the *strata*, are distinguishable, and even distinct in the *lava*, though variously disfigured. Another observation I made, and which appears to me very interesting, is, that most of these branches, in the Veronese and Vicentine territories, whether marine, volcanic, or mixed, still preserve nearly the same external characters, directions and parallelism, exclusive of the trifling alterations produced at the surface of the latter, as I before observed. It seems, therefore, sufficiently evident, that fire not only operates locally on lapideous solids, but often also in such a manner as not intirely to destroy all marks of their primary organization and qualities, much less to alter their dispositions, and the external characters of the masses or mountains, they form. And though all traces of the primary organization of these masses may be effaced by new modifications, yet often sufficient proofs remain of their former characters, in the forms, direction, and disposition of the mountains they compose, as appears from the instance I have just considered, and is still more strongly confirmed by the phænomena of Auvergne and Velay, which I shall consider upon a future occasion. It does not, therefore, seem impossible, nor even difficult, to trace the leading original character of a country, though it has suffered by fire a new modification of the *strata* that compose it. And the volcanic mountains before described not only afford evident marks of their having pre-existed

in another state, but manifest also plainly their primary qualities and structure, and equally prove, by their particular directions, that they never can have been thrown up fortuitously, from the bowels of the earth, like the Monte di Cenere, &c.; but have suffered fire *in statu quo*, or locally, without the least appearance of subversion, or change of place. The same seems also very probable of many other mountains, that are purely volcanic, from the necessity of the local origin of their particular organizations, which I have before considered; and supposing even that such mountains manifest no internal nor external signs of their primitive structures or qualities. From the preceding observations it appears, I think, evident, that subterraneous explosions and eruptions are merely accidental phænomena, that are by no means essential to the production of all volcanic mountains, as has been commonly imagined. This notion seems to have proceeded from the affinity often observable in the qualities and external forms of many such mountains, and those of real volcanos. But I have already observed, that there are many volcanic mountains of a totally different form from the common volcanos; and though their *lava* may sometimes be similar in its qualities, yet the regular organization of it, in the former, frequently makes a most essential difference. For, as I before said, what does Vesuvius or Ætna present to us but a heap of ruins, which give us not the least idea of the structures to which they belonged? And though they may lead and enlighten the chymist, yet they afford but little instruction

tion to the physical geographer, a constant sameness of phenomena occurring every where. Nor have we any foundation, from the external appearance of such mountains, to conclude, that all others, that have suffered fire, are of the same character. We see nothing but a heap of ruins, cast up from their bowels, and we are apt to imagine, that such inordinate materials compose the intire mass; and analogy, too often seductive in similar matters, leads us to conclude the same of other vulcanic mountains in general. But I am much inclined to think, that the materials thrown up by burning mountains, are only lodged superficially, as it were, on their sides; and though they may considerably increase their bulk, as well as alter their form, yet they do not seem to constitute the intire mass of those mountains, as might be reasonably imagined from their external appearance. For it has been observed, both by PADRE DE LA TORRE, M. DE LA LANDE<sup>(g)</sup>, and others, that the inner sides even of the funnel of mount Vesuvius preserve manifest vestiges of its primary organization, in regular, parallel, and nearly horizontal *strata*, like those of other common mountains. And does it not appear more than probable from hence, that an original mountain lies under the *lava* of Vesuvius, serving in a manner, as its base, and which, whatever local alteration it may have received intrinsically, from the subtle element, that wastes its bowels, still maintains its primary undisturbed structure, like the vulcanic mountains of the Veronese territory be-

(g) Voyages d'Italie, tom. VII. p. 169, 176.

fore described. It is not therefore likely, that the whole of mount Vesuvius should have been made at several times, by the earth and cinders flung up out of the mouth, that lies in the midst of them, the ashes falling down the sides of it, like the sand in an hour-glass, as Mr. ADDISON<sup>(b)</sup> particularly observes, and as most people are naturally enough apt to think. And however considerably the bulk of this mountain may have been increased, by the loads of *lava* thrown out at the several eruptions; yet no great addition seems to have acceded to its height, if the observation just mentioned be true; which, though I have never had it in my power to verify, yet I readily believe; not only on account of the respectable authorities above quoted, but also from its consistency with other similar and indisputable phænomena, of which the vulcanic branch of Montebello before described affords, if I mistake not, no inconsiderable proof. Nor does it indeed appear agreeable to reason, that mountains of great height, where volcanos exist, should be entirely vomited up in this manner; such volcanos seeming to require a solid and permanent channel for the violence and frequency of the effects they produce. It seems therefore, that a way or channel only has been opened, by an original effort or explosion, through mount Vesuvius, which has since been deeply buried by the materials successively thrown up, from its bowels, in the several eruptions. And I am inclined to think the same of mount *Ætna*, from its superior height, though I am uncertain

(b) Travels, p. 184.

whether BORELLI, or any other of its historiographers, takes notice of any fact, by which this conjecture is proved. The same seems also probable, for similar reasons, of mount Hecla, the Pic of Tenerif, &c. And if this be true of single or isolated volcanos, like Vesuvius and Ætna, that carry such strong external marks of having been exclusively produced by the accumulated *lava* of eruptions, much more is it likely to be so of others, that are not only considerably higher, but form parts of a continued chain, like the volcanos of the Andes, as I imagine. Is it not, moreover, highly improbable, that Chimboroso, which is one of those volcanos, and the highest mountains in the known globe, measuring, according to the French academicians, 3220 toises, which exceed four Italian miles, allowing 764 toises to a mile; is it not, I say, highly improbable, that such a mountain should exclusively owe its origin and formation to the accumulated materials of eruptions only? May not the same be said of the mountains Antisana, Kotopacsi, Pichincha, and the other volcanos of the Andes, of which some are little inferior in height to Chimboroso, and constitute in general the highest parts of that vast chain? And though the summits of these mountains, in all probability, form isolated points, yet, I presume, they unite in an uninterrupted mass below, like other chains of mountains. And if this be true, it is hardly credible, that the masses where these volcanos respectively exist, can have been indebted solely to them for their origin, or that they can have been fortuitously cast up from the

bowels of the earth like the Monte di Cenere, the island of Santorino, &c. If they form integral parts of a continued chain, as it is natural to suppose, is it not even absurd to imagine, that they can have had such an origin? Is it not, on the contrary, rather to be presumed, that channels only have been opened along this chain, by different explosions, where these volcanos respectively exist; and that the sides of these channels form integral parts of its original structure, as in the case of mount Vesuvius before remarked, and which here seems to receive the strongest confirmation. For however the eruptions of the volcanos of the Andes may have loaded their sides and summits in particular parts; yet surely inferior masses exist of a much prior origin, and whose continuity sufficiently seems to prove, that such eruptions are, relatively, only accidental phænomena. This reasoning is, however, grounded upon the supposition, that the Andes form a continued chain, like that of other similar mountains; and, as I presume, they do. From the preceding observations it seems therefore evident, that whether volcanic mountains preserve, or not, vestiges of their ancient and primitive state, or in whatsoever manner they appear to have been newly organized; few of them seem to have been intirely thrown up from the bowels of the earth, like the Monte di Cenere, &c. On the contrary, they mostly appear to have pre-existed in another state; and to have suffered by fire only locally, and more or less partially, of which I have given sufficient proofs; or, having afforded only passages to explosions and eruptions,

are partly in an original state within, and partly increased by new and adventitious materials superadded to their surface by successive eruptions; as seems to be the case of the volcanic mountains of the Andes, mount Vesuvius, and probably of most other volcanos of any great height; more especially where they form parts of continued chains. And if so few of the extinct volcanic mountains appear to have been thrown up, from below the common surface of the earth, like the Monte di Genere, &c. nor even those intirely which manifest actual volcanos; it seems highly improbable, that other common mountains should have had such an origin; as many respectable writers <sup>(i)</sup> have been inclined to think; and still more so, that such should have been the sole origin of all mountains; as a late Italian writer <sup>(k)</sup> on the theory of the earth has very unsuccessfully endeavoured to prove. It also plainly appears, if I mistake not, from what has been before said, that the phænomena of recent volcanos are very little calculated to give us much instruction about the more curious igneous concretions, and the origin of volcanic mountains in general; and that a few days tour in such countries as Auvergne, Velay, and the Venetian state are worth a seven years apprenticeship at the foot of mount Vesuvius or Ætna; where nothing but a heap of un-instructive ruins, and a sameness of phænomena appear. And since our ideas, concerning volcanic effects, have

(i) HOOKE's Philosophical Discourse on Earthquakes in his posthumous Works. RAY's Physiological Discourse. RASPE Specimen Globi Terræquei.

(k) MORO de Crostacei che fu i Monti si trovano.

been almost exclusively drawn from recent volcanos, we cannot much wonder if they yet remain so imperfect.

Having dwelt a little, in the course of this paper, on the physical geography, and particular volcanic phenomena of Auvergne, Velay, and the Veronese and Vicentine territories; I shall beg leave to add a few observations of the like nature relating to the Euganean hills; more especially as they produce other volcanic concretions equally curious, and of a very different character from any observed in the provinces before mentioned. The Euganean hills form an irregular group in the plain of Lombardy, about seven miles nearly south by west from Padua, and extend from north to south as far as Este. The most considerable part of them composes an irregular sort of chain, which extends in the above direction; while other parts are severally detached, and form isolated mountains about the skirts of this chain, particularly on the north-east side, towards Abano. The outer skirt of the entire group may measure perhaps from thirty to forty English miles. The external characters of this group exactly correspond with the forms commonly ascribed by naturalists to volcanic mountains in general; since the points of the chain before mentioned, as well as the isolated members of it, are of various conical, orbicular, and elliptical shapes. As this group, therefore, rests upon a perfect plain, it makes a very singular appearance, and exactly answers to the following lines of Ovid<sup>(1)</sup>, which, I hope, I may therefore be permitted to insert, though in a philosophical paper.

(1) Met. lib. XV.



*Extentam tumefecit humum, ceu spiritus oris  
Tendere vesicam solet, aut directa bicornis  
Terga capri; tumor ille loci permansit, et alti  
Collis habet speciem, longoque induruit ævo.*

The vulcanic hills immediately round Isenchaux in Velay affect also the same forms; but as they are mixed with other hills of a different form, and the country about them is broken and irregular, they do not produce so singular an effect as the Euganean hills, which suddenly rise from a perfect level. I am informed, that there is a similar, though smaller group of isolated vulcanic hills in a plain of Dalmatia, near Cossovo; and another group of hills, nearly of the same forms, in the county of Down, in Ireland, and called the Mourn Hills; which, like those near Padua, consist, as I am informed, mostly of granite and *lava*. The Euganean hills have, moreover, a superficial and partial covering of stony and calcareous *strata*, of posterior origin, and that manifest no marks of having suffered by fire. Such *strata* slightly capmount Venda, which is the highest among these hills; though of no very considerable elevation, measuring only about 252 French toises above the Venetian Lagoons, according to abbé Toaldo, professor of astronomy at Padua, who lately, at my request, obligingly took its elevation from the observatory at Padua. From the *lava* and granite mixed together in the Euganean hills, they bear an affinity with those of Auvergne and Velay; but differ from them by the superincumbent unburnt *strata* of Lime-stone. This they call Scaglia, or Scagliola, from its

its being composed of thin flaty *strata*, which are of a yellowish colour, and contain a few vestiges of fossil marine bodies, but no regular bed of them. Sometimes an irregular mass of marble is found among the Scaglia, and near Arqua particularly, of a most beautiful kind, much resembling the noted Florentine figured marble; with this further addition, that, besides the ruins it represents, it is also variegated by frequent dendrites, which, if I mistake not, are very uncommon in the former. Part of the tabernacle of the great altar in the church at Arqua is of this marble, which also takes a most beautiful polish. If Vaucluse, near Avignon in France, is become celebrated from the memory of the plaintive and eloquent PETRARCH, Arqua ought still to be more so; since not only his remains lie there, in a large *sarcophagus*, of red Veronese marble, in the church-yard; but his villa at Arqua is still in being, and preserves some pastoral and historical fresco paintings, of himself and his LAURA, of no inconsiderable merit. His great armed chair, and the skeleton of his favourite cat, are also still in being. This villa was his retreat, during his residence at Padua, where he was a canon of the cathedral. The common *lava* is not so frequent among the Euganean hills as in the provinces of Auvergne and Velay, and seldom forms intire hills, which are mostly of granite, on the surface of which, where there is no lime-stone, the *lava* is partially and superficially scattered, and sometimes mixed even with the mass of granite. I have already observed, that the prismatic group of Monte Rosso is nearly of the same sort of

F 2

granite

granite with the hill, and is the only one in those parts; the common vulcanic tracts affording nothing similar. Having dwelt sufficiently on this group, in the beginning of my paper, I shall briefly add a few observations on the physical topography of Monte Rosso itself, with an account of some other of its vulcanic productions, not less curious than the group of columns before described.

Monte Rosso is isolated from the principal chain of the Euganean hills, of an orbicular form, and measures about a mile and a half in circumference at the base. It consists principally of gray granite, which is disposed in blocks, and irregularly perpendicular *strata*. I have already mentioned the quality of this granite, and its resemblance with that of Auvergne and Velay. Towards the surface of Monte Rosso, it appears sometimes rotten, or friable, and porous, as it were, like a motley kind of *lava*, which I have frequently seen. But I was surprised, on examining the granite masses of Monte Rosso, to find in them pieces of common porous brown *lava*, which did not appear to be casually lodged there, and of extraneous origin (like the rounded pebbles in pudding stone, and other aqueous *strata*) but manifestly seemed to form integral parts of the mass itself, and to have concreted with it at the same time. I observed a similar sort of porous *lava*, but of a black colour, in the granite of the castle hill at Moncelese, near Este, at the south east skirt of the Euganean hills; and I doubt not but the same is common to others. This fact, added to many others, which I shall not insist upon at present, seems strongly

to confirm an opinion, which I have long entertained of the igneous origin of granites in general; some further proofs of which I shall hereafter consider. A dingy red ochrous earth covers partly the surface of Monte Rosso, from whence probably proceeds the name given to the hill; *rosso*, in Italian, signifying *red*. Much iron sand also abounds here, as it commonly does about other volcanic and granite mountains or tracts in general. Among the figured concretions of Monte Rosso, I observed a small open perpendicular bank, at the east end of the hill, which presented a group of a very peculiar structure. It is formed by an aggregate of angular bodies, laterally ranged together, like balsantine columns, but in a horizontal direction, with their tops in front, and prominent, as they are represented in the figure<sup>(m)</sup>. These prominencies are of a globose form, and made rough by a number of small crystallizations, of a parallelepiped figure, that are concreted in the mass, which is of a yellowish colour, and rather friable sort of volcanic substance, in so much that I could not separate, or isolate the bodies so far as precisely to determine their particular form, though a correspondent continuation of the external angles appears within, and they seem to contract a little, pyramidically, like the isolated body figured<sup>(n)</sup>, and which seems to be somewhat of the same kind, but of a much harder substance. This is also from a part of Monte Rosso near the prismatic columns. Though, as I before said, there are very few of the Euganean hills that intirely

(m) Fig. 2.

(n) Fig. 3.

consist

consist of common basaltine, or other *lava*, like those of Auvergne, Velay, and the Vicentine and Veronese territories; yet some there are, and very curious, which I shall briefly describe. Monte Nuovo, or the New Mountain, about a league to the south of Abano, and near Battaglia, is one of them. Though connected with the principal chain of the Euganean hills, by a depressed neck, or isthmus, yet it contains no granite, or lime-stone, like the rest of that chain, but is formed exclusively of *lava*, of various kinds, and different from any *lava* I observed about these hills. Great part of its surface, especially about the top, is very rough, knotty, and sinuous, and manifestly appears to have concreted from a fusion by fire. The skirts and bottoms of it equally prove the same, consisting mostly of another sort of mixed and congealed *lava*, which the Italians call *lava brecciata*, from its resemblance to the Breccia marbles. It is formed by many broken and irregular fragments, that have been accidentally licked up, as it were, or collected by the *lava*, while melted and running, and concreted with it, without however suffering fusion. Large broken masses of this *lava* are seen about the foot of the hill, very much resembling fragments of ancient Roman buildings ruined; inasmuch, that I mistook for such the first mass I saw, under a part of the hill, called Il Monte della Croce, from the church built upon it. The manner in which the Romans were accustomed to fill up the inner parts of their thick walls, within the facings, exactly resembles this kind of Breccia *lava*; since they used irregular bits

bits of stone, confusedly thrown into, and cemented with a large body of mortar. This kind of *lava* is common enough about Vesuvius, and other recent volcanos; and also occupies large tracts in Upper Auvergne, especially between Murat and Aurillac; where it covers the skirts of the hills, by the side of a valley, for many miles; forming the most grotesque figures imaginable. I have also observed it, in plenty, in the environs of Puy, in Velay. The famous St. Michael's church at Puy is built on the summit of a high, isolated, and almost pointed rock of this *lava*, and makes a very extraordinary appearance. Monte Nuovo seems to have suffered a more recent conflagration than any other of the Euganean hills, as its name rather implies. But though its skirts and surface appear, almost every where, to have suffered a fusion; yet parts of the internal structure of the mountain, manifest, however, the primary horizontal direction and parallelism of the *strata*, as I before remarked of the Veronese volcanic hills. This I particularly observed within the park of Cataio palace, which is situated at the north-east extremity of Monte Nuovo, and has parts of its under apartments cut out of the solid rock of *lava*. Supposing Monte Nuovo isolated from the low narrow isthmus before mentioned, it would measure about six miles in circumference at the base. I shall further observe, that it forms a sort of half moon, on the west side of the plain near Battaglia, and stands in the centre of several hot springs; as those of St. Elena, and San Bartolomeo, to the south and west; those of Abano and Monte Ortone to the north,

north, and many others. The neighbourhood of Monte Nuovo is also rendered interesting to naturalists by its fossil glass, *vitrum obsidianum* PLINII, *pumex vitreus solidus* LINNÆI, which is found in plenty under the west side of it, in a small valley, called Val San Zibio, near the baths of San Bartolomeo. It exactly answers to the characters given of it by LINNÆUS and others; as may be seen by some specimens of it, which I have sent, and which were broken from a very large mass. It abounds about the volcanos of the Andes particularly; but I found none of it in Auvergne, Velay, or the Veronese or Vicentine volcanic hills. Monte Castello, or the Castle Hill, near Baon, at the south-east skirt of the Euganean hills, and about half a league from Este, affords another volcanic concretion of a very remarkable structure. This hill is mostly formed of huge oval and laminated masses<sup>(c)</sup>, of various sizes, confusedly concreted together, like a pudding stone, but in a volcanic matrix, consisting of a sort of dark-brown stone, with angular *lapilli* in it of a dingy-whitish colour, and visibly manifesting an affinity with the ordinary granite of the other neighbouring hills; and perfectly similar to the figured masses concreted with it. In other parts, and particularly at Monte Galda, a small isolated elevation in the plain of Padua, between the Euganean and Vicentine hills, I have found these laminated masses of a spherical figure. In a broken cavity of a mixed volcanic and marine hill of Monte Galda, I saw a group of these laminated round balls regularly placed one above the other, and

(c) Fig. 4.

perpen-



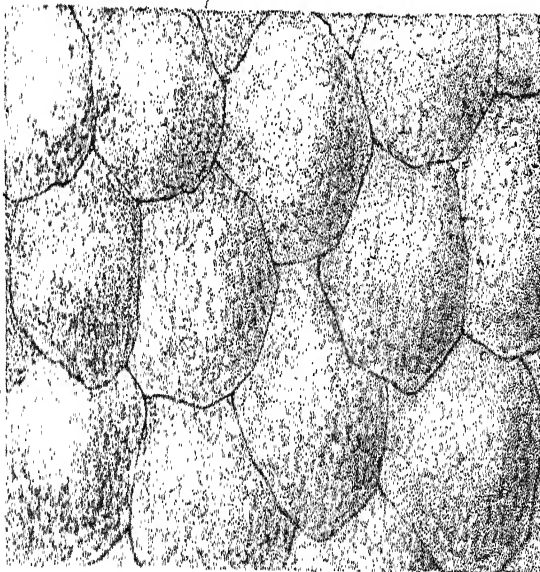




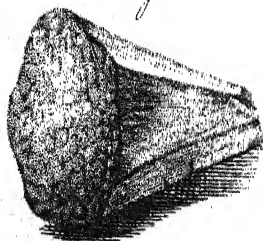
*Fig. 1*



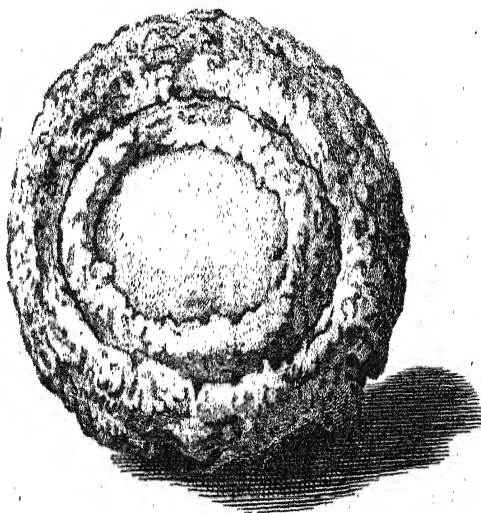
*Fig. 2.*



*Fig. 3.*



*Fig. 4.*





perpendicularly, in columns, as it were. The annexed figure (*p*), though sketched chiefly from memory, may serve to give some idea of this phænomenon. I have often observed the granite, of the Euganean hills particularly, affecting such orbicular and laminated forms; as may be seen in the figure (*q*) representing a perpendicular section of a similar bank in these hills. Indeed, from this and other facts, which I have occasionally mentioned in the course of my paper, there seems to be a strong analogy between granites and many particular volcanic concretions. Another of the Euganean hills, called Monte Uliveto, or the Mount of Olive Trees, near Teolo, is chiefly composed of a confused aggregate of smaller globular volcanic balls, which are rather solid than laminated, and of a hard ferruginous substance, of a dark-brown colour, much resembling some common ferruginous *geodes* I have seen. The annexed figure (*r*) represents a group of them. This may suffice for a specimen of the more curious volcanic productions of the Euganean hills, and united to the observations before made, may perhaps show, how little we are acquainted with the structure and relative phænomena of volcanic bodies in general. I shall not enter into any account of other more common productions of this kind; and shall only mention, that, during my summer's residence at the baths of Abano, I made a collection of the Euganean *lava*, which is now in the public museum at Padua. Since such productions are rather calculated to illustrate the physical topography of the coun-

(*p*) Fig. 5.(*q*) Fig. 6.(*r*) Fig. 7.

try to which they belong, than to adorn a cabinet, they must be more useful upon the spot, than they possibly can be elsewhere; which consideration will, I hope, serve as my apology for not having transmitted them to the Society with the other specimens. It seems rather extraordinary, that so curious a tract as that of the Euganean hills, which differs from all others in Italy, should have remained so long unknown, especially being in the neighbourhood of frequented baths, and a celebrated university. For though BACCIUS, in his book *De Thermis*, supposes subterraneous fire, to account for the heat of the waters of Abano, and also mentions pumice stones about the baths there; yet he means only the porous and calcareous concretions formed by deposition from those waters; nor does he seem to have been at all acquainted with the indisputable volcanic phænomena, which the adjacent hills in plenty afford. Nor does VANDELLI enter into any observations of this kind, in his more modern and express treatise *De Thermis Patavinis*; if we except the mention of the fossil glass of Val San Zibio, which he seems to have first observed. I therefore hope, that the foregoing observations will be the more acceptable. It appears also from hence, if I mistake not, that the Venetian territory in general abounds full as much with volcanic phænomena, as any other part of Italy. For, besides my observations in the Paduan, Veronese, and Vicentine territories, I have seen *lava*, in the public museum at Padua, from the Brescian hills; and have observed *lava* pebbles in quantity in the beds of the rivers  
flowing

flowing from the mountains of Friuli. I therefore doubt not but these provinces also abound with vulcanic phenomena, though I have never had an opportunity to visit them. I mention these circumstances more willingly, since it has been generally imagined, that the northern parts of Italy contained few, if any, such productions. They are, however, not only full as common, but, if I mistake not, more instructive than those of any other province of that country. For, besides the phenomenon of Monte Rosso, on the importance of which I have already insisted, and the other curious vulcanic productions of the Euganean hills, I must observe, that, from the inordinate course of the Appenines in general, the vulcanic hills of that chain afford no observation so interesting to physical geography and the theory of such phenomena, as that before remarked of the correspondent direction and parallelism of the vulcanic and other branches of the Veronese and Vicentine districts. My observations also, on the vulcanic branches of these districts, do not seem to agree with the celebrated MONTGUETTARD's principle, who supposes<sup>(s)</sup>, that all vulcanic materials observable in calcareous countries are adventitious; the contrary of this being indeed demonstrated by the facts I have advanced. Nor have I entered particularly into an account of my vulcanic tour in the Venetian state, that I might not abuse myself of the sufferance of so respectable a Society by an uninteresting detail of

(s) *Memoire sur la Mineralogie d'Italie*, in the first volume of his *Memoires sur les Sciences et les Arts*.

facts, however justifiable I might have been, by the importance of some of them. For another imperfect basaltine columnar group, of which I have also a drawing, exists near Gambellara, in the Veronese territory, a few miles from Montebello; and, according to the informations I have received, they are equally prevalent in the adjacent district of Vicenza. Doctor FESTARI, an ingenious physician of Valdagno, whose curiosity I have fortunately excited in these matters, lately informs me, that he has discovered a similar group of prismatic columns, in the mountains of that neighbourhood; and I had before been apprized of another near Maso, not far from Bassano, by Mr. ARDUINI, a celebrated naturalist of Venice. I have observed fragments of prismatic columns about Maso, but did not see the group. When I was at San Giovanni Illarione, I was also informed, that, at some distance, by the side of the torrent below, another similar group existed. Nor have I the least doubt of the frequency of such phænomena, especially in the Vicentine hills, where volcanic effects are more common even than in the neighbouring territory of Verona. For of all the numerous lines, or branches, of mountains, that diverge from the chain of the Alps, and intersect, nearly in parallel directions, the Vicentine district, there is not one, I believe, but what contains more or less *lava*, and in quantity; whereas many of the said branches, in the Veronese territory, are exclusively marine and calcareous; especially in the immediate neighbourhood of Verona, about the Val Pantena, the Val Policella, and towards the

the Adige; and the Veronese mountains, that mostly abound with *lava*, are those of the east and north-east quarter, about the confines of the Vicentine territory, in the common road from Verona to Vicenza. I first perceived vulcanic effects in the environs of Caldiero, where the hot springs rise. The immediate hills about them, which are isolated in the plain, though of little elevation, are almost exclusively vulcanic; as are likewise the neighbouring points of the Alpine branches before mentioned. None of the writers on the baths of Caldiero take any notice of these facts, though they seem almost inseparable from the consideration of the origin and properties of those waters. The same defect is observable also in the writers on the hot springs near Viterbo, which are in the centre of vulcanic hills, and on the other *Thermæ* near Radicofani, in the confining part of the Tuscan state, where, as I before observed, vulcanic effects also abound. This neglect indeed is but too common to other writers on *Thermæ* and mineral waters in general; physical topography seldom forming a part of their inquiries, however pertinent and even necessary in itself.

Having often had occasion to speak of Abano in the course of this paper, I cannot conclude it without mentioning an extraordinary phænomenon in the animal kingdom, which is observable there. Notwithstanding the heat of those waters, in which FAHRENHEIT's thermometer rises to eighty-eight degrees, a particular species of *buccinum* breeds and lives in them, and is found in great plenty. It is of the fluviatile kind, and seems to be peculiar



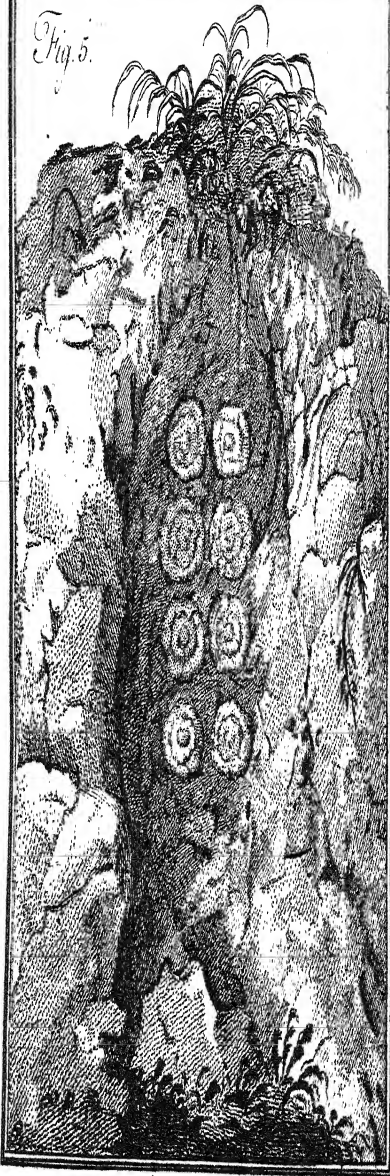
peculiar to these waters, having never seen nor heard of them in any others. They are remarkably small, scarcely exceeding a line or two in length, and are perhaps the smallest univalve or testaceous animal of any such kind hitherto discovered. It is mentioned and figured by VANDELLI in his treatise *De Thermis Patavinis*; but the figures are not good, and much too large; as may be seen by the original specimens herewith sent.

Such are the observations, which I have the honour to present to you, SIR, and to the other learned members of the Royal Society upon the present occasion. I shall think myself very happy if they afford any satisfaction; and more particularly so, should they be found conducive to the advancement of so interesting a province of Science as that of Physical Geography, which being grounded upon facts, that require observation, seems hitherto to have suffered for the want of it. If, contrary to the common opinion, I have insisted on the local origin of most volcanic tracts, it may further be considered, that this seems full as consistent with the principle of their origin, as it is agreeable to the phænomena themselves. For fire not only penetrates, pervades, destroys, and new modifies the texture of the most solid bodies; but is also often generated in these bodies, without the previous intervention of other fire; which consideration alone might lead us to the opinion I have advanced, were there not such evident proofs in support of it. These will, I hope, receive a stronger confirmation from a more particular account of the volcanic phænomena of Auvergne and Velay,

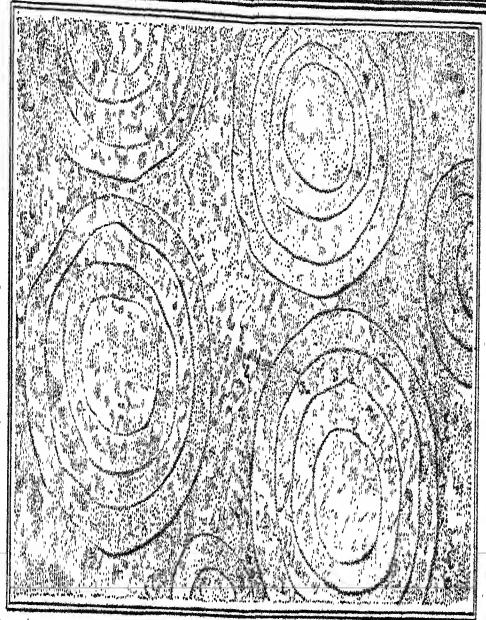




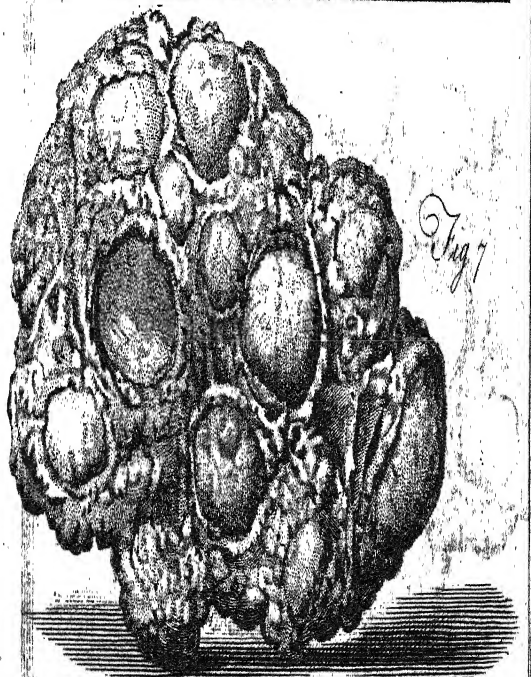
*Fig 5.*



*Fig 6.*



*Fig 7.*





Welay, which I propose myself the honour of presenting to the Society upon a future occasion. In the mean time I am, with great respect and esteem, SIR,

Your most obedient

and very humble servant,

JOHN STRANGE.

P. S. Since the termination of the foregoing paper, I am informed by Mr. CHARLES HAY, of Brecknock, in South Wales, that the prismatic basaltine columns of Towen before mentioned were actually brought from Cader Idris, at the top of which mountain there are prodigious quantities of them, which are totally different from the rocks around them. This information Mr. HAY has lately received from a person informed; having obligingly made the inquiry at my request. I may, perhaps, be enabled hereafter, through the same channel, to transmit to the Society a more particular account of the phænomenon; presuming, that, in some part or other of Cader Idris, these columns form a regular group, as they commonly do in other places.

III. *An Inquiry to show, what was the ancient English Weight and Measure according to the Laws or Statutes, prior to the Reign of Henry the Seventh.*

Redde, Nov. 24,  
1774. **W**ILLIAM the Conqueror, by his charter, confirmed to the English all their ancient laws, with such additions or alterations as he made therein, to their advantage. The 57th clause of that charter is, “*De mensuris et ponderibus. Et quod habeant per universum regnum, mensuras fidelissimas et signatas, et pondera fidelissima et signata sicut boni prædecessores statuerunt.*” From this clause it seems clear, that king WILLIAM ordained, sealed standards both of weights and measures, to be made, such as his predecessor king EDWARD had ordained. Neither weights or measures are here described particularly; but the subsequent statutes define them more plainly. And the *Chronicon Pretiosum* tells us, that from historians it appears, the Conqueror determined what the weight of the sterling penny, or penny weight, should be, to weigh 32 grains dry wheat. Consequently the standard penny weight was made equal to the weight of 32 grains wheat. Succeeding kings confirmed WILLIAM’s charter; and even the great charter granted by king JOHN, is only to explain and restore the ancient

ancient laws, which had been infringed. The statutes of 51st of HENRY III. and 31st of EDWARD I. explain the ancient weights and measures; that is to say, the English penny called a sterling, round without clipping, was to weigh 32 grains dry wheat, taken from midst of the ear, and 20. of those penny weights were to make an ounce, and 12. ounces a pound; and 8 of those pounds were to be a gallon of wine, and 8 of those gallons to make a London bushel, which is the  $\frac{1}{8}$ th part of a quarter. The definition of the penny weight in these statutes agrees with the determination of WILLIAM the Conqueror, and shows the legal weight continued the same. What the weight of that pound was, so raised from a penny weight, equal to the weight of 32 grains of wheat, we may clearly learn from that declaration in the 18th of HENRY VIII. when he abolished that old pound, and established the Troy weight; which says, that the Troy pound exceedeth the old Tower pound by  $\frac{3}{4}$  of the ounce. As the Troy pound established by HENRY VIII. is the same as is now in use, consisting of 5760 Troy grains, and 480 grains to the ounce, and 12 ounces to the pound: so 360 grains is  $\frac{3}{4}$  of the ounce, which, deducted from 5760, leaves 5400 Troy grains, equal to the weight of that old Saxon pound which he abolished. But to trace out experimentally the weight of that penny weight, raised from 32 grains of wheat, I got a small sample of dry wheat of last year 1773 (the wheat of that year but ordinary); and, from a little handful taken therefrom, I told out just 96 round plump grains, dividing them into

Vol. LXV. H parcels



parcels of 32 grains each, and all three weighed exact  $22\frac{1}{2}$  Troy grains; consequently, 240 such penny weights, which the old pound consisted of, were equal only to 5400 of our present Troy grains, conformable to the declaration of HENRY VIII. Thus the weight of that old pound is clearly ascertained to be lighter than the present Troy pound by  $\frac{3}{4}$  of an ounce; and it clearly shews, they were two different weights. By those statutes of HENRY III. and EDWARD I. it is said, that 8 pounds were to make a wine gallon, and 8 of those gallons to be a bushel, and 8 bushels a quarter; consequently the wine and corn gallon were one and the same measure. The statute of the 12th of HENRY VII. says, the gallon measure was to be 8 pounds of wheat, which ascertains what was to be understood by former statutes, and is consonant to reason, to fix the measure of wheat by its own weight, not by that of wine, as wheat was an article of greater importance to the community to ascertain its measure than wine; and a gallon measure to contain 8 pounds of wheat, must be  $\frac{1}{4}$  part larger in cubical contents than a measure to contain 8 pounds of wine. As it appears by the charter of WILLIAM the Conqueror, that there were sealed standards made of weights and measures, we cannot doubt, but they were preserved and kept in the king's exchequer, for legal standards; and as several statutes direct their being made of metal, they were permanent and certain, whereby to make more: which HENRY VII. expressly tells us he practised, by making new according to the old: so that there could be no need to re-

cur-

cur to 32 grains of wheat, much less to 7680, every time new standards were to be made, unless we suppose our ancestors defective in common sense. Whenever, by new statutes, fresh standards were directed to be made, we may observe, the assize of weight and measure continued uniformly fixed and described to be one and the same, to shew there was no alteration made or intended. And thus, by the laws of Assize, from WILLIAM the Conqueror to the reign of HENRY VII. the legal pound weight continued a pound of 12 ounces, raised from 32 grains of wheat, and the legal gallon measure invariably to contain 8 of those pounds of wheat, 8 gallons to make a bushel, and 8 bushels a quarter; the bushel, therefore, contained 64 of those pounds of wheat, and the quarter 512 pounds. These were the legal weights and measures for common use, during that period. The first alteration, really made therein, was in the 12th year of HENRY VII. as will be mentioned hereafter. That the laws of Assize were often infringed, is very evident from the frequent complaints, mentioned in COTTON's Abridgement of the Tower Records, against the king's purveyors; particularly in the 14th of EDWARD III. for remedy against outrageous takings of purveyors; and in the 45th of EDWARD III. that the king should be served by *common measure*; and in the 3d of HENRY V. that the king's purveyors do *take 8 bushels of corn only, to the quarter striked*. The general answers whereto were, that the statutes should be observed. It appears also, that others infringed the laws of assize. For the statute of 27th of

EDWARD III. says, Some merchants bought Avoirdupois merchandizes by one weight, and sold by another; which plainly implies, they bought by some weight heavier than the legal, and sold by the legal weight which was lighter; for it is rather too absurd for supposition to imagine, they bought by a light weight, and sold by a heavier. The statute, therefore, to enforce observance of the laws of assize, only wills and establishes, that there be, *one weight, one measure, and one yard, through all the land*. This can be understood to mean no other, than the legal assize, which preceding statutes had enacted. And farther, in the reign of HENRY VI. we see that buyers of corn, bought by a vessel, called a fatt, of 9 bushels, which contained 72 gallons; and like those merchants before mentioned in the statute of EDWARD III. we may presume they sold by another measure, the legal quarter of 8 bushels, containing but 64 gallons: for the statute of 9th HENRY VI. forbids the buying by that vessel, called a fatt. The prohibition implies the illegality of the vessel and its use, and implies also the enforcement of the laws of assize. Taking therefore all the several statutes together, in one connected view, those that fix the laws of assize, with those to reform abuses committed against them, we are led to conclude, those laws of assize continued uniformly one and the same, till HENRY VII. altered them. Having thus shewn by those laws, that the old pound weight was a Saxon pound of twelve ounces, raised from 32 grains of wheat, and was equal only to 5400 of our present Troy grains; and that the measure of capacity was  
a gallon,

a gallon, to contain 8 of those pounds of wheat, and 8 of those gallons made a bushel: I shall now endeavour, by help of figures, to demonstrate what was the cubical contents both of the gallon and bushel measures.

We know, the present Troy pound consists of 5760 Troy grains, and that 7000 of those Troy grains are equal to the present Avoirdupois pound of 16 ounces, and that 5400 of those Troy grains are equal to the old Saxon pound of 12 ounces; consequently, the old Saxon pound was  $\frac{540}{576}$  of the present Troy pound, and the old Saxon pound was  $\frac{54}{70}$  of the present Avoirdupois pound. We know, modern experiment hath proved the weight of 1728 cubic inches of wheat, common sort, to be  $47\frac{1}{2}$  pounds Avoirdupois; and of a better sort, to weigh from  $48\frac{1}{4}$  to  $48\frac{1}{8}$  pounds Avoirdupois, the difference in their weight is not very great; however, I will take the lowest weight to compute by, the  $47\frac{1}{2}$  pounds Avoirdupois, which, in Saxon weight, is  $61\frac{3}{4}$  pounds Saxon. And then I say, as  $61\frac{3}{4}$  pounds Saxon : 8 pounds Saxon :: 1728 cubic inches :  $224\frac{1}{2}$  cubic inches, for contents of the old Saxon gallon for wine and wheat. But as the old standard wine gallon, kept at Guildhall, and found there in 1688, proves to be 224 cubic inches contents, there is reason to conclude it to be of the same standard affize, as was the ancient Saxon gallon for wine and wheat: for, as 1728 cubic inches : 224 cubic inches ::  $61\frac{3}{4}$  pounds Saxon :  $7\frac{5}{8}$  pounds Saxon, which is about  $4\frac{1}{2}$  penny weights short of the 8 pounds, mentioned in the statutes for the gallon to contain, and is such a small difference, as may arise in different

rent years, in the weight of such a quantity of wheat. The very near agreement of these computations, gives us sufficient reason to conclude, that the old standard wine gallon, of 224 cubic inches contents, found at Guildhall in 1688, was of same standard affize, as was the ancient gallon measure ordained to hold 8 Saxon pounds of wheat; and of course then, the bushel measure must have been 1792 cubic inches contents, which will appear to hold nearly 64 Saxon pounds of wheat, as by those old statutes it ought to do. For, as 1728 cubic inches : 1792 cubic inches ::  $61\frac{31}{54}$  pounds Saxon :  $63\frac{1476}{1728}$  pounds Saxon, which is only about an ounce and three quarters short of 64 pounds; and in so large a quantity of wheat, is a trifling difference, naturally arising in weight of wheat of different years. These demonstrations, by figures, sufficiently prove, what the cubical contents of those ancient English measures must have been, according to the old statutes of affize; that is to say,

The gallon measure, 224 cubic inches contents, to hold 8 pounds Saxon.

The bushel, 1792 ditto, 64 ditto.

And as 8 bushels made a quarter, the quarter contained 512 Saxon pounds of wheat. These were the ancient legal measures, according to the old laws of affize.

It now remains to mention the particular statute of the 12th of HENRY VII. under which, an alteration was brought about in those ancient weights and measures, without seeming to intend it; as the statute itself differs not in substance from the other old laws of affize, except calling the pound by a new name, Troy. But previous thereto,

thereto, it may be necessary to observe, that very probably, the unfettled state of the kingdom for many years preceding, might pave a way to that alteration. There had been several contests about the crown, between the two houses of York and Lancaster, till HENRY VII. by conquest, mounted the throne; and in such times of public disturbance, the laws of assize were more likely to be infringed, than well kept. For, after HENRY VII. was well settled on his throne, we find complaint was made in the 11th year of his reign, that the laws of assize had not been observed and kept. Whereupon he made fresh standards of weights and measures, and sent them to the several shires and Towns in the kingdom. But in the very next year (the 12th of his reign) there came out that particular statute, under which, the weights and measures were altered. Reciting, that the king, in the former year, had made *weights and measures of brass*, according to the *old standards thereof, remaining in his treasury*, which weights and measures are said, on a more diligent examination, to have been *approved defective*. It is not said, whether they were the old standard weights and measures, or the new ones, made in the former year, that had been approved defective; nor how much they were so: all this is left to conjecture. Therefore we may, with great probability, conjecture, they were not defective in respect to their old original standard; but only in respect to the heavier new Troy pound, intended to be then introduced. And what warrants such conjecture is, the express declaration of his son HENRY VIII. when he  
abolished

abolished the old pound, in the 18th of his reign, and established the Troy; for he then declares, the Troy pound exceedeth the old pound by  $\frac{3}{4}$  of an ounce. This sets the matter in a clear light, and shews what the two weights were, and what the difference between them. Hence then, there can be no doubt, but HENRY VII. altered the old English weight, and introduced a heavier Troy pound, that exceeded the old one by  $\frac{3}{4}$  of an ounce; and although none of his standard weights have come down to us, yet his brass bushel measure, with his name upon it, was found in the Exchequer in 1688, and proves to be 2145 cubic inches contents; from which we may form conclusions, both on his weights and measures, sufficient to convince us, that he altered both. That his bushel was a measure of 9 gallons instead of 8, and that his Troy pound was  $\frac{1}{16}$  part heavier than the old English pound, which was raised from 32 grains of wheat. Experiment hath proved, that a measure of 1728 cubic inches of wheat, will weigh from  $47\frac{1}{2}$  to about  $48\frac{1}{4}$  pounds Avoirdupois; but suppose it be only  $47\frac{3}{4}$  pounds Avoirdupois, that, in Troy weight, will be  $58\frac{17}{576}$  pounds Troy. From hence we may easily find the weight of wheat that 2145 cubic inches will contain. For, as 1728 cubic inches : 2145 cubic inches ::  $58\frac{17}{576}$  pounds Troy : 72 pounds Troy, the weight of wheat that HENRY VIIth's bushel would contain. And dividing the 72 by 8, the number of pounds limited by the statute to a gallon, it proves HENRY VIIth's bushel was a measure of 9 gallons instead of 8; and as 8 bushels made a quarter, then the quarter contained 72 gallons.

gallons; this seems to correspond with the number of gallons contained in the vessel, called a fatt, the use of which was prohibited by statute in HENRY VITH's time, about 60 years before HENRY VII. as hath herein been already remarked. . If we divide the 2145 cubic inches contents of the bushel, by 9, the number of gallons it contained, it shews the gallon measure to be  $238\frac{1}{3}$  cubic inches contents, which is  $\frac{1}{16}$  part larger than the old Saxon gallon of 224 cubic inches, just in the proportion as the Troy pound is  $\frac{1}{16}$  part heavier than the old Saxon pound. The statute limits the gallon to hold 8 pounds Troy of wheat; and so we find the gallon of  $238\frac{1}{3}$  cubic inches will do: for as 2145 cubic inches :  $238\frac{1}{3}$  cubic inches :: 72 pounds Troy : 8 pounds Troy. But if it be said, the statute limits the bushel to 8 gallons, not 9, then the gallon measure must have been  $268\frac{1}{8}$  cubic inches contents, and would hold 9 pounds Troy of wheat, though the statute says it was to hold but 8 pounds Troy. Take it either way, it shews that the bushel was not made according to the statute; it held 72 pounds instead of 64 pounds. And upon the whole it clearly proves, that HENRY VII. altered both the weights and the measures; that he introduced the Troy pound, which was heavier by  $\frac{3}{4}$  of an ounce than the Saxon or old English pound; and that his bushel measure was about  $\frac{1}{6}$ th part larger than the ancient Saxon or old English bushel measure. The first statute that directs the use of the Avoirdupois weight is, that of the 24th of HENRY VIII. which plainly implies it was no legal weight, till that statute gave it a legal



gal function; and the particular use to which the said weight is there directed, is simply for weighing butchers meat in the market. And it is note-worthy, that in all the old statutes of assize prior to HENRY VII. the legal gallon measure of capacity is founded on 8 pounds, raised from the weight of 32 grains of wheat, and by that statute of 12th HENRY VII. the gallon is to contain 8 pounds Troy: therefore, these two sorts of weight were the only ones established as legal by the statutes; and both are a lighter weight than Avoirdupois. How, or when, the Avoirdupois weight came first into private use is not clearly known to us; but this seems clear, that no statute before the 24th HENRY VIII. hath given it any legal sanction.

IV. *The Description of an Apparatus for impregnating Water with fixed Air; and of the Manner of conducting that Process.* By John Mervin Nooth, M. D. F. R. S.

Redde, Dec. 15, 1774. **T**HE possibility of impregnating water with fixed air was no sooner ascertained, by experiment, than various methods were contrived to effect the impregnation. The ingenious Dr. PRIESTLEY, however, is the only one that has published any description of an apparatus, calculated intirely for this purpose. This apparatus was communicated to the public, with the view of promoting the discovery of the medical effects of fixed air united with water; and, in consequence of this communication, some very successful attempts have been made in the cure of diseases. The experiments, however, have not been so numerous as one could have wished; perhaps the difficulty in conducting the process, in the manner proposed, has been, in some measure, the reason why so few experiments, on this subject, have been made public. For although, in the hands of the doctor, the apparatus was sufficiently convenient, it must be confessed, that the conduct of the process required more address than generally falls to the share of those that are unaccustomed to such experiments. Independent too of the inconveniences attending the process, there was another

I 2

objection

objection to the apparatus, which, with most people, might have considerable weight. The bladder, which formed part of it, was thought to render the water offensive; and when the solvent power of fixed air is considered, it will not appear improbable, that the water would be always more or less tainted by the bladder. In some trials which I made with Dr. PRIESTLEY's apparatus, it always happened, that the water acquired an urinous flavour; and this taste in the water was, in general, so predominant that it could not be swallowed, without some degree of reluctance. The difficulty, therefore, in the conduct of the process, and the offensiveness of part of the apparatus, made some less exceptionable method of producing the impregnation desirable. This I variously attempted, keeping convenience and cleanliness constantly in view; and I flatter myself, that I have at last contrived an apparatus that will perfectly answer the intended purpose. It is now twelve months since this contrivance has been in constant use; and hitherto there is no reason to wish for the least alteration. Presuming, therefore, on the possibility of its becoming, when known, extensively useful, and convinced of the favourable reception which every attempt of this nature meets with from the Royal Society, I beg leave to communicate to them a description of the apparatus that I have invented, and of the manner of conducting the process.

## DESCRIPTION OF THE APPARATUS;

which is of glass, and consists of three vessels as (A, B, C), fig. 1. 2. 3. The glasses are accurately fitted to each other, and at the joints are impervious both to air and water. The glass (A) is designed for the effervescing substances. The vessel (B) is to contain the water that is to be impregnated with air. In the lower part of the glass (B) is placed an ivory valve, surrounded with cork, as in fig. 4. The cork (*a*) is fitted to the bottom of the glass (B), and has through it an hole, to receive the part (*b*) of the ivory valve. On the broader part of this piece (*b*) is placed a moveable piece (*c*). The surfaces of these pieces are so accurately ground, that, when applied to each other, no fluid whatever can pass between them. The moveable part (*c*) is secured on the part (*b*) by the cover (*d*), which is so constructed, as to allow the piece (*c*) some motion; and this cover has likewise holes to give passage to the air that shall raise the moveable piece (*c*). The glass (C) serves two purposes; it confines the air on the surface of the water in (B), and at the same time prevents all danger of explosion by allowing the water to give place to the ascending air.

## THE PROCESS.

As chalk and oil of vitriol are capable of producing the desired effervescence, and are the most eligible on account of their cheapness, I shall, in describing the process, mention only these two ingredients. Variety of other substances

substances may, however, be employed for the same purpose; but none, perhaps, are so unexceptionable as those I have named. In the other acids a proper degree of fixity is wanting, during the effervescence; the nitrous and marine have so much volatility that there is always a risk of some of the acid fumes passing the valve, and thus rendering the water acid, which it was intended to impregnate only with fixed air. To begin the process, it is necessary to fill the vessel (A) up to the dotted lines, with diluted oil of vitriol. By confining the height of the surface of the effervescing mixture to the dotted lines in the glass (A), none of the acid will be driven through the valve, during the intumescence that attends the escape of the fixed air. The glass (B) is to be totally filled with water, and the vessel (C) is to be put on it. Some powdered chalk is then to be thrown into the glass (A), and the vessels are to be immediately placed as in fig. 5. except that the stopper belonging to (C) is to be left out. When the acid, in the lowermost vessel, acts on the chalk, the extricated air passes the valve in the middle glass; and as the construction of this valve allows the fixed air from the effervescing substances to pass, but denies a passage to the water in a contrary direction, the separated air ascends to the upper part of the middle glass, and at the same time a portion of water, equal in bulk to the intruding air, passes up the bent tube into the uppermost vessel. As the effervescence goes on, the fixed air continues to accumulate in the middle vessel, and the uppermost one to be filled

filled with the water that has given place to the air. The quantity of chalk to be thrown into the acid at one time, must be determined by the capacity of the uppermost vessel. Should more air be extricated than is sufficient, in the conduct of the process, to fill that vessel, the water will run over the top of it, and will continue to run as long as any air ascends in the middle vessel, or till the surface of the water is below the extremity of the bent tube. Both these accidents are to be carefully avoided; as in one case, the whole would be wet and disagreeable; and in the other, a quantity of fixed air would be unnecessarily lost. Half a dram of chalk will, in general, produce air enough to fill the uppermost vessel with water; and it must be remembered, that the chalk employed to produce the effervescence, should be finely powdered, as a selenitic crust will otherwise form around it, and thus prevent the action of the acid on the interior part. To keep the neck of the glass clean, through which the chalk is put, it will be necessary to include the chalk loosely in paper; and this circumstance is by no means to be neglected, as the accurate junction of the glasses depends on it, and consequently the whole of the process. When the uppermost vessel is filled with water, and there is, therefore, a considerable quantity of fixed air in the middle one, these two vessels are to be separated from the lowermost, and the air and water are to be agitated together, to promote their union. If, during the agitation, a stopper be put into the uppermost glass, the descent of the water in it will not shew the absorption of the fixed

2

air

air by the water, as the external atmospherical air will enter below, at the valve, to fill the space which the absorbed fixed air would otherwise leave void. But, on the contrary, if the uppermost vessel be open, during the agitation, the pressure of the atmosphere on the surface of the water in that vessel, will force the water down into the middle one, as fast as the absorption of the fixed air below will allow it room. This latter method may be pursued, when a person wishes to know the quantity of fixed air that the water can absorb; but in common use, it will be better to stop the uppermost vessel, as the air and water may be then more forcibly agitated without inconvenience, and of course, the impregnation more expeditiously effected. During the effervescence, the uppermost glass is to remain open, and it is only to be stopped when the agitation is performed. It is not to be expected, that the impregnation will be considerable at first; it will indeed be necessary to repeat the process, with the same water, four or five times, before it will be highly impregnated. After an agitation, therefore, when a stronger impregnation is wished for, the uppermost vessel is to be opened, and raised from the middle one, to allow the water to descend, that was before driven up. When the middle glass is again full, a fresh quantity of chalk is to be put into the lowermost vessel, and the agitation to be repeated, as soon as the effervescence ceases. It is seldom necessary to repeat the process more than four times; to produce a very strong impregnation; but should it be thought proper, to have the water as highly

2

saturated

aturated with fixed air as it admits of, nothing more than a repetition of the same process is requisite. In this account of the apparatus, I have purposely confined myself to the method of uniting fixed air with water; but it is to be observed, that many curious experiments may be made with it, both in chemistry and pharmacy. By its assistance, I have been enabled to imitate very perfectly, the common mineral waters, and to make aqueous solutions of substances that were before deemed insoluble in water. These circumstances, however, I shall reserve for a future paper, which I shall have the honour to present to the Society, as I have not yet been able to arrange the several facts, which this apparatus has made me acquainted with, in the manner I could wish.

## P O S T S C R I P T.

SINCE the foregoing paper was read, I have contrived a glass valve, which seems preferable in some respects to the ivory one therein described. The following is a description of it. It consists of three pieces, as in fig. 7. The superior and inferior pieces are perforated, but the middle one is without perforation, having only its upper part convex and its under part plane. In fig. 8. is a perpendicular section of the three pieces composing the valve, at the distance at which they ought to be placed, with respect to each other, in the tabular part of the vessel (B). This vessel having the glass valve in it, and filled with water, is to be put on the glass (A),



containing substances in the act of effervescence. In that case, the extricated air will ascend through the perforations in the superior and inferior pieces, the middle one proving no obstacle to the air, having sufficient room to yield to the current of air rushing upwards; but when the air ceases to ascend, and the pressure of the water above takes place, the middle piece will prevent the water from descending, its plane surface being then applied to the plane surface of the piece below it. Thus, SIR, this glass valve will answer in every case where the ivory one can be employed; and for a variety of purposes it will undoubtedly prove preferable, particularly when corrosive substances are subjected to experiment.







Fig. 5.

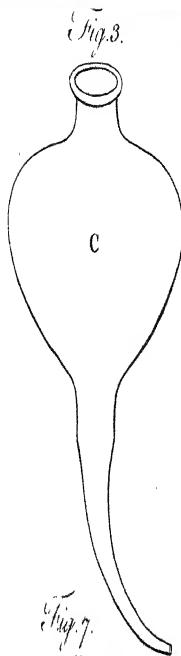
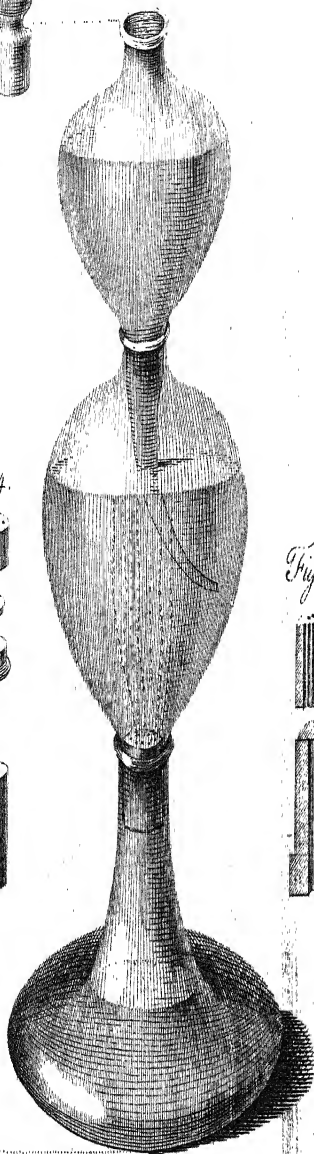


Fig. 2.

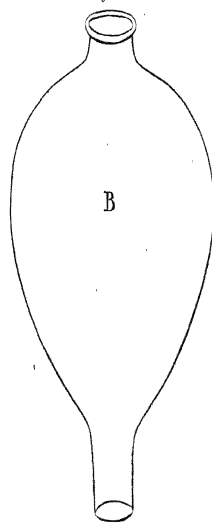


Fig. 4.



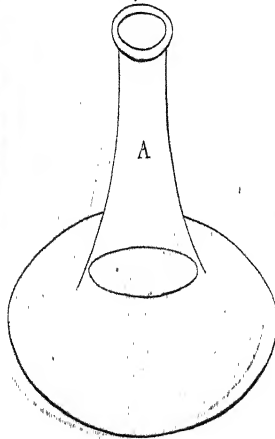
Fig. 8.



Fig. 7.



Fig. 1.





V. *Account of a Musical Instrument, which was brought by Captain Fourneaux from the Isle of Amsterdam in the South Seas to London in the Year 1774, and given to the Royal Society. By Joshua Steele, Esquire, in a Letter to Sir John Pringle, Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

SIR,

Margaret-Street, Cavendish-square,  
December 13, 1774.

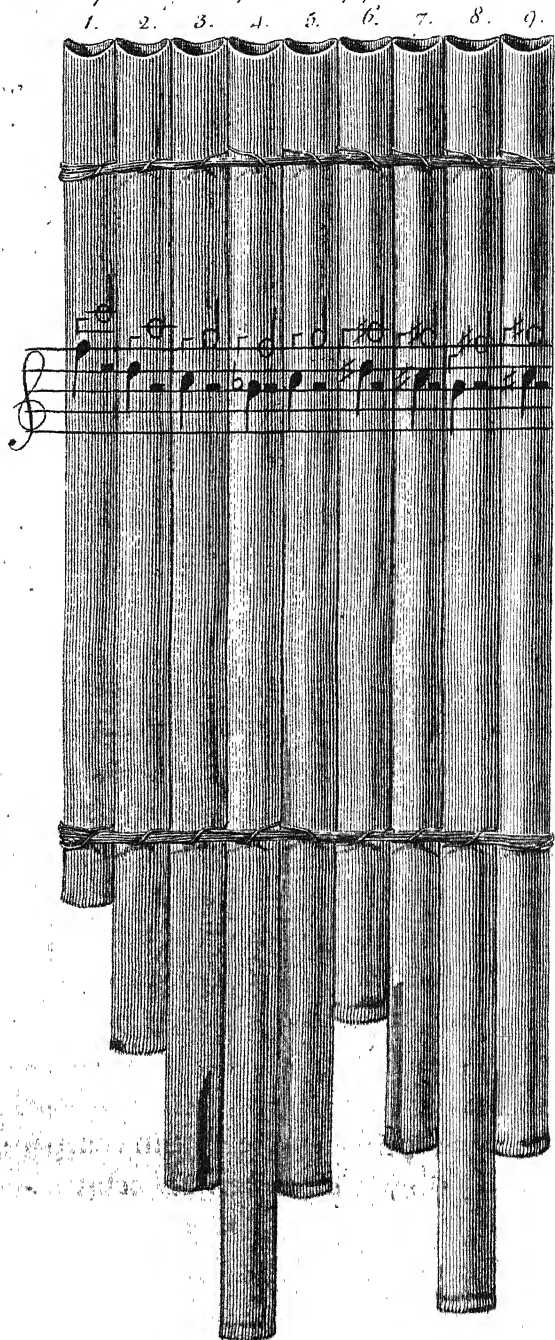
Redde, Jan. 22,  
1775.

**A** GREEABLE to your request, I have examined the curious system of pipes, brought by Captain Fourneaux from the South Seas. The result of my experiments are herewith inclosed. The instrument was so new to me, that I should be sorry its reputation should rest intirely on my report, as I think an expert blower of the German flute might make further discoveries; towards which, my observations, whether perfectly accurate or not, may in some measure serve as a guide. The accident of a flat third, coming in the stead of a sharp one, from the pipes 6. 7. 8. and 9. is so extraordinary, that I suspected, for some time, the lowest (or fundamental) tones of those pipes were a quarter tone (or *diefsis*) lower than I have marked them; but, after repeated trials, and by the best judgement I could form by my ear, and by comparison with another instrument, I gave

up that fuspicion;  
and being confirmed in the opinion, that the most acute tones I could obtain from those four pipes, were *minor thirds* to the most grave, I have ventured to mark them so. The reason why there was room for my doubt above mentioned is, because the difference of hotter or colder, moister or dryer, has a sensible effect on the acuteness or gravity of the tones.

I am, SIR, with  
great regard,  
Your most humble servant,

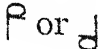
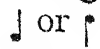
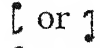
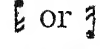
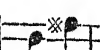
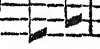

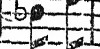
JOSHUA STEELE.

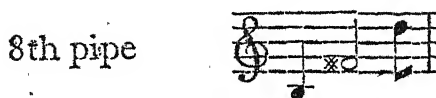
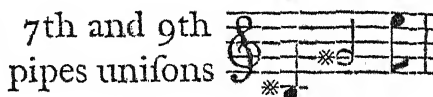
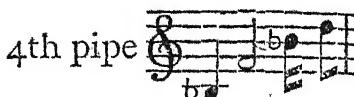
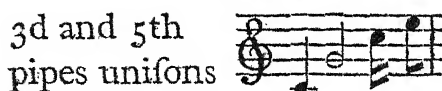
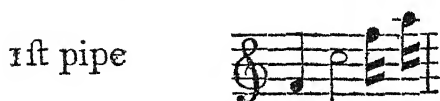



Explanation of the system of musical pipes, brought from the Isle of Amsterdam in the South Sea, by Captain Fourneau, to London, *anno* 1774, from experiments made by Mr. STEELE.

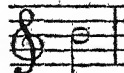
The manner of blowing these pipes, in making these experiments, was the same as people use to whistle in the pipe hole of a drawer key. Of the tones, marked on the drawing, the upper series, which are exact fifths to the lower, are easiest produced by an unexperienced person; and the lower series, which we will call fundamentals, with somewhat more address and a weaker blast. Beside the above mentioned tones, if the velocity of the breath be increased a little, the five first pipes will give octaves to the fundamentals; and if farther increased, sharp thirds, or tierces, above these octaves. In the pipes 6. 7. 8. and 9. I could neither make the octaves to the fundamentals, nor the sharp tierces; but in their stead, the minor, or flat-third, above the octave came, when the breath was urged beyond the degree requisite to produce the fifth. This minor third is an accident out of the natural order of tones produced from simple tubes, which I do not pretend to account for. Here following, are set down the notes of the several tones which I produced from each pipe; but, in order to bring them more within compass of the scale of five lines, they are written an octave lower than they

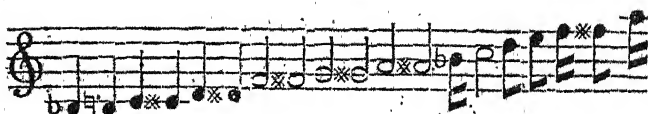


really are on the pipes. And also those tones which come with most ease are wrote in minims, as  or   
 those in the next degree, in crotchets, as  or   
 those still more difficult, in quavers, as  or   
 and the most difficult in semiquavers, as  or 



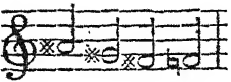
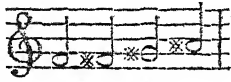
These tones are adapted to English *confort pitch*, by the above notes. From whence it is evident, that an expert performer may exhibit the following series, and perhaps also the octave to the fundamental ; *vi-*

*delicet*, , though I could not, which series is sufficient for an infinite number of airs:



In this series the notes marked in minims, being those which are easiest to be founded, furnish two systems which correspond with the definitions of the diatonic

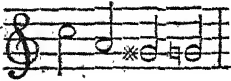
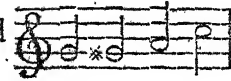
and chromatic *genera*, according to Euclid, who describes the diatonic in descending, κατὰ τόνον, ἢ τόνον, ἢ ἡμιτόνιον; and in ascending, καθ' ἡμιτόνιον, ἢ τόνον ἢ τόνον.

as  descending, and  ascending.

Interval of  
a tone,  
and a tone,  
and  
a semitone.

Interval of  
a semitone,  
and a tone,  
and a tone.

And the chromatic thus, κατὰ τριημιτόνιον, ἢ ἡμιτόνιον, ἢ ἡμιτόνιον, in descending; and ascending, καθ' ἡμιτόνιον, ἢ ἡμιτόνιον, ἢ τριημιτόνιον.

as  descending, and  ascending.

Interval of  
3 semitones,  
and 1 semitone,  
and 1 semitone.

Interval of  
1 semitone,  
and 1 semitone,  
and 3 semitones.

But as the enharmonic *genus* requires intervals of the *diesis*, or quarter tone, and as it did not appear by these experiments, that the pipes could exhibit any sounds by such intervals, I conclude they are not capable of performing according to the enharmonic division of the tetrachord.

VI. *Remarks on a larger System of Reed Pipes from the Isle of Amsterdam, with some Observations on the Nose Flute of Otaheite.* By Joshua Steele, Esquire.

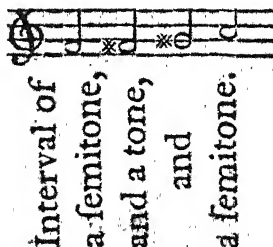
TO SIR JOHN PRINGLE, BART. P. R. S.

SIR,

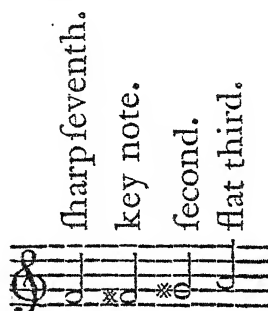
Margaret-street,  
Feb. 21, 1775.

Redde, Feb. 22,  
1775.

THE notice taken of my small endeavours, by your illustrious Society, does me much more honour than I deserve; however, I receive it, as I ought, with respect and gratitude. I now inclose to you such farther remarks as I have been able to make, by repeated trials, on the last reed pipes you brought me from Mr. BANKS; which, though much larger, and more in number, are of the same *genus* with the former. I have also examined the nose-flute of Otaheite, which Mr. BANKS favoured me with; and I find it gives only four sounds, with the first degree of breath, which are, in an ascending series, by a semitone, a tone, and a semitone. Thus noted in consort-pitch,



If urged with a stronger breath, it will give octaves above these; but it then becomes ill in tune: and I understood from Mr. Banks, the natives of Otaheite use no more than those first four sounds. Were I to give these notes denominations according to our system of music, they should be distinguished thus,



Notwithstanding the small extent of this series, yet, by the aid of varying the measure, it is capable of several different melodies, though the general cast of them will be melancholy. As for example,



adapted to the nose-flute, are, harmonically, the same,

though rhythmically different; the latter having a degree of vivacity more than the former, in proportion to its measure of time; two bars of the first, being equal, in length, to three of the second.

I am, SIR, with great regard,

Your very humble servant,

JOSHUA STEELE.

Remarks on the larger system of reed pipes from the isle of Amsterdam.

The specific difference between this and the smaller system, described before, will be understood from the following observations. It consists of ten pipes, joined together in the same manner as those of the smaller system. The first nine pipes exhibit to the eye the same figure as the system before described in the drawing; and the tenth pipe (which is the additional) is a little longer than N<sup>o</sup> 4. For in this larger system, N<sup>o</sup> 8. is thirteen inches long; N<sup>o</sup> 4. thirteen and a half, nearly; and N<sup>o</sup> 10. is fourteen inches. The sounds which each pipe *exhibits easily*, are marked in *minims*, as follows, and are noted agreeable to concert pitch:



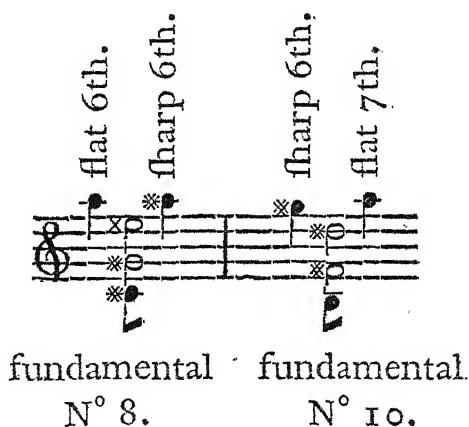
As

As the upper minims are fixths to those next under them, it follows, from the law of harmonic sounds, that the lower minims are fiths to the fundamental sounds of these pipes, which are written in quavers, to shew that they are very difficult to be produced. The upper minims of N° 1. 2. 3. 4. 5. and also of 10. are sharp thirds, or rather, major tenths, to the fundamental sound of each pipe. And the upper minims of N° 6. 7. 8. and 9. are nearly minor tenths to their fundamentals; which circumstance seems to agree with what I remarked in the smaller system, as an extraordinary property, touching the minor thirds. † But I will not yet assert, that this property is altogether natural, because I found some of these latter pipes were partly obstructed by accidental rubbish, which was drawn out with difficulty; so that I pretend not to decide, whether the cause of their being, not quite, in the same proportion of tune, as I found in the first system, arises from some casual injury, or from original intention, or original inaccuracy. ‡ I have said, the upper minims of N° 6. 7. 8. and 9. are *nearly* minor tenths to their fundamentals; because, in fact, I found them something more than *minor*, and yet not *major*; wherefore I have used the mark (※), of a triple cross, to signify something more than (\*), the double cross; and the mark of (×), a single cross, to signify a *diesis*, or something less than (\*), the double cross; which last, in the modern practice of music, always means to say, *plus a semitone*, neither more or less. For though

† and ‡ referred to from the following page.

the nicety of the *diefs* is stealing insensibly into the fancy of fingers, and of some other elegant musical performers, it is not as yet adopted, or used as such, in the notation of modern music. The interval between N° 1. and 2. in these pipes, is only of two semitones; whereas, that between the N° 1. and 2. of the former system, was of three semitones. The series N° 2. 3. 4. and 5. and the series N° 6. 7. 8. and 9. (both of which I have distinctly marked within bars) have similar intervals in both systems (making allowance for what I have said in page 75, † and ‡.) Wherefore I imagine these to have been the original extent of the whole modulating series, like the double tetrachord of the Greeks, and that the N° 1. and N° 10. are additional at pleasure; as, in the smaller system, the interval between N° 1. and 2. was a semitone greater than that between N° 1. and 2. in the larger system; and N° 10. in the smaller system (first examined) was totally omitted, though I have seen two others which had it. The sounds in this larger system are seven tones lower than those of the smaller, which corresponds with the difference of their dimensions; the pipe N° 4. in this system measuring nearly thirteen inches and a half in length, with diameter seemingly proportional; whereas the N° 4. in the smaller system measured only seven inches and a quarter. By increasing the velocity of the blast, I found these pipes gave sounds still higher, which were *fourths* above the upper minims, or *octave and sixths* above the fundamentals; and with a little more force, *tritones*, or *sharp fourths*,  
above

above the upper minims, which were *octave and flat sevenths* above the fundamentals. But these two (the 4th and sharp 4th above the upper minims) should rather be considered as one *note of latitude*, which by more or less velocity, or force of breath, makes in the N° 1. 2. 3. 4. 5. and 10. either a sharp 6th, or a flat 7th, to each of the fundamentals; or in the N° 6. 7. 8. and 9. either a flat or a sharp 6th.



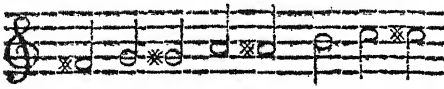
This note of latitude is common to all tubes, trumpets, horns, &c.

The following notes mark the ascending series of the sounds of this larger system, omitting the fundamentals, and giving only those which are more easily obtained.

The numerical figures shew from which pipe the notes were produced.

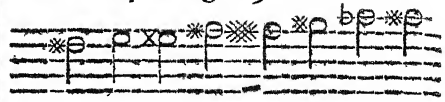


3. 7.  
N° 10. 4. 8. 5. 9. 2. 6. 1.



Fifths above the supposed  
fundamentals, produced  
by a gentle blast.

3. 7.  
N° 10. 4. 8. 5. 9. 2. 6. 1.



Tierces, or tenths, above  
the supposed funda-  
mentals, produced by a  
stronger blast.

VII. *Description of a new Dipping-needle. By Mr. J. Lorimer, of Penfacola, in a Letter to Sir John Pringle, Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

SIR,

Penfacola, Sept. 13, 1773.

Redde, Feb. 2,  
1775.

WHENEVER any one meets with a *terrella*, or spherical loadstone, the first thing he does is to find out its poles; and having once discovered them, he knows immediately how any small bit of needle will be affected, if it is placed upon any part of the surface of that *terrella*. The poles are most readily discovered by trying where the filings of iron, or a small bit of needle, will stand erect upon the *terrella*; and this is generally found to be upon two points which are diametrically opposite to one another. But the magnetic poles of the earth seem to be situated obliquely to one another (see the Berlin Memoirs, 1757); but where they are actually situated is hitherto unknown; whether they are upon land or water; or in either case, whether we can come nigh to them. Yet be these things as they may, it appears evident to me, that accurate observations, made as near to these magnetic poles as possible, with a good dipping-

dipping-needle, are the surest way to complete the magnetic theory of this globe, analogous to the method we pursue in examining the *terrella*. But as all the dipping-needles which I had seen, appeared to me to be very ill calculated, for the sea service at least, I contrived one upon a different plan in 1764, and had it executed before I left England, by Mr. sisson. I have called it an Universal Magnetic Needle, or Observation Compass; because I can by it take the dip and amplitude, and even the azimuth, with only one assistant, to take the altitude for me. The needle is of the same shape and size nearly as those used now for the compasses of the royal navy, and plays vertically upon its own axis, which has two conical points, slightly supported in two corresponding hemispherical <sup>(a)</sup> sockets, which are inserted into the opposite sides of a small upright brass parallelogram, about one inch and a half broad and six inches high. Into this parallelogram is fixed, at right angles, a slender brass circle, about six inches diameter, silvered and graduated to every half degree, upon which the needle shows the dip, by a *vernier* if you choose; and this, for the sake of distinction, I shall call the circle of magnetic inclination. This brass parallelogram, and consequently the circle of inclination, also turns horizontally upon two other pivots, the one above and the other below, with corresponding sockets in the parallelogram. These pi-

(a) Mr. sisson thinks, that these sockets were conical as well as the ends of the axis, but more obtuse than them; which seems most likely to be the case, as they seem much more likely to answer well than hemispherical sockets.

vots are fixed in a vertical brass circle, of the breadth and thickness of two-tenths of an inch, and of such a diameter, as to allow the circle of inclination and the parallelogram to move freely round within it. This second circle I shall call the general meridian. It is not graduated, but has a small brass weight fixed to the lower part of it, to keep it upright; and the circle itself is screwed, at right angles, into another circle, of equal internal diameter, of the same thickness, and twice the breadth, which is silvered and graduated on the upper side to every half degree. It represents the horizon, as it swings freely upon gimbols, and is always nearly parallel to it. The whole is contained in a neat mahogany box, of an octagon figure, with a glass plate at top and one on each side, for about two-thirds down. That part of the frame which contains the glass lifts off occasionally. The whole box turns round upon a strong brass center, fixed in a double plate of mahogany, glued together cross-ways, to prevent its warping or splitting; and this again is supported by three brass feet, such as are used for the cases of table knives, frosted that they may not easily slip, if the vessel should have any considerable motion. It has another square deal box to lock it up in, to preserve the glass, &c. when it is not wanted for use.


The use of this instrument is very plain, as the inclination or dip is at any time apparent from inspection only, and also the variation, if the frame is turned round till the great vertical circle lies exactly in the plane of

the true meridian : for the circle of inclination, being always in the needle's vertical plane, the edge of it will evidently point out upon the horizon, the variation E. or W. But at sea, when there is not too much motion, you turn the frame round, till the vertical circle is in the plane of the Sun's rays; that is, till the shadow of the one side of it just covers the other, and the edge of the circle of inclination will then give the magnetic amplitude, if the Sun is rising or setting; but the azimuth at all other times of the day, and the true amplitude or azimuth being found in the usual way, the difference is the variation. If the motion is considerable, observe the extremes of the vibration, and take the mean for your magnetic amplitude or azimuth. When the Sun does not shine so bright as to give a shadow, you can set the brass circle in a line with his body, if he is at all visible by your eye. The principal advantage at first aimed at in this compass, was to contrive a dipping-needle, which should be sufficient for making observations at sea. As those needles, to be of use, must be placed, by some means or other, in such a manner as that all their vibrations shall be made in the true magnetic meridian; North and South, otherwise they are good for nothing. For if one of them is placed at right angles, across the magnetic line, it will stand perpendicularly up and down in any part of the world; the least dip, therefore, is always in this magnetic line. But the only method of setting a dipping-needle at sea, was to place it in a line with the common compass needle; and this must be  
 very

very inaccurate, if they are at any considerable distance one from the other; or if they were near, the two needles would influence one another, and neither of them could be true: nay, supposing them for once to be properly placed in this line, the least motion of the ship throws them out again. But this instrument has a constant power in itself, not only of setting itself in the proper position, but also of keeping itself so; or of restoring itself to the same situation, if at any time it has lost it; and it is curious to see how, by its double motion, it counteracts, as it were, the rolling motion of the vessel. I have only one thing farther to observe, that as it is impossible for human hands to make any instrument mathematically true, so when we have two graduations to look to, as in the present case, one on the North, and the other on the South of the needle, we ought to attend to both, and take the medium for the true dip or variation pretty nearly. But in this compass there is another method of examining the observations. Take a good artificial magnet, and on the outside of the compass-box, point one end of it towards the needle, and by moving your magnet you may thus guide the north-end of the needle round to the south; or, *vice versa*, without opening your compass-box. The magnet being then laid aside, the needle will come to its true position, after a few vibrations: but as both the needle and the circle of inclination are now reversed<sup>(b)</sup>, it will not point exactly to the

(b) Mr. LORIMER means, that the magnet should be applied in such manner as to turn the parallelogram and circle of inclination half way round horizontally, so that that end of the axis of the needle which before pointed to the west, shall now point to the east.

same division as before; yet a mean of the two will be the truth, as nearly, I believe, as it is possible for any instrument to give it.

Quere 1st. May not a part of this small difference be attributed to the direction of the magnetic influence (whatever that be) in the steel bar? and if such an experiment could be tried upon the present azimuth compasses, is it not probable, that the variation in them would be at least as sensible? Quere 2d. May not this be the cause that two of the best of them will differ a small matter from one another? Quere 3d. Would the ends of the needle being made thus , instead of the square form be, in some measure, a remedy for this small variation?

I am, most respectfully, SIR,

Your most obedient humble servant,

J. LORIMER.

VIII. *Bill of Mortality for Chester for the Year 1773.* By  
J. Haygarth, M. D. F. R. S.

Redde, Feb. 2, 1775. **T**HAT Chester is healthy to a very remarkable degree, is still more clearly evinced from the following tables, than in the register of last year. In 1772, one half of the inhabitants appeared to arrive at 20 years of age; a fact which seemed very surprising when compared with the proportional mortality in other towns, both of a larger and less size. But, according to this year's register, one half have lived to be 36 years old. In 1772, one in 15 and 3-4ths had lived to above 80, and this year 1 in 13. These are very uncommon instances of longevity for so large a proportion of the inhabitants. The inhabitants of St. Michael's parish were numbered to be 618, of whom this year ten have died; that is, a less proportion than 1 in 61. If the inhabitants of the whole city were numbered with the same accuracy as those of St. Michael's, many important conclusions, both medical and political, might with certainty be deduced from the bill of mortality. The register of burials in the nine parishes are kept separate; hence, by comparing the number of inhabitants in each parish with the burials in each, for a period of years, we may, on the most evident foundation, discern which



which part of the town is most healthy. In a political view, such an account would furnish the best means of demonstrating the accuracy of a table of the probabilities of life, formed from the register, and supply unerring *data* for calculating annuities, the value of reverſionary payments, and affurances on lives. Such an old town as Cheſter, where the number of inhabitants has for many years ſuffered little variation, and where the births and burials are nearly equal, is peculiarly well fitted to furniſh this important information. At the requeſt of Dr. PRICE, author of the very ingenious eſſays on annuities, &c. an improvement is made in the firſt table, by continuing the diviſion of lives into periods of five years, from 50 till 80, and from that age to the extremity of life, by marking exactly the number dying in every year; becauſe for want of ſuch a register, the law, according to which life waſtes after 80, is at preſent almoſt totally unknown, and the values of annuities on ſingle and joint lives, incapable of being calculated with any tolerable exactneſs, beyond 70 or 75. The following tables confirm the obſervation, that women live longer than men. Of thoſe who have lived to above 80, only 10 are males, and 17 females; the number of widowers this year is 17, of widows 44. The table of diſeaſes of different ages (N<sup>o</sup> II.) confirms in general the obſervations of laſt year. It is evident that no epidemic viſited this place in 1773; not one died of the meaſles, or miliary fever, and the 10 who ſunk under the chinkcough had probably lingered under the diſeaſe ſince

since the former year, towards the end of which it ceased to be epidemic. Only one has died of the natural small-pox; twelve were inoculated in Chester, during this year, and all recovered. In order to determine the utility of inoculation, it is necessary to ascertain, by an induction of facts, to what proportion of those who are infected, this disease is fatal in the natural way. I have received a very authentic account of the following fact, which is perhaps the more curious and instructive, as no medical practitioner whatever visited any of the patients during the whole disease: at Kelfall and Ashton, two small Cheshire villages in this part of the county, on an eminence of a dry absorbent sand rock, 69 persons have had the small-pox during the last seven months; of whom 12 have died, that is, 1 in 5 and 3-4ths. In confirmation of last year's observation it is proper to remark, that between the ages of 15 and 50, more have died of consumptions this year, than of all other diseases.

The III<sup>d</sup> table shews, at one view, what diseases were most fatal in each month.

TABLE I. Deaths, Ages and Conditions.

Ages.	Males.	Females.	Ages.	Batch- lors.	Huf- bands.	Wi- dowers.	Maids.	Wives.	Wi- dows	Total.
Under 1 month —	6	4	20-25	6	1		4	1		12
Between 1-2 months	11	5	25-30	5	3		1	6		15
2-3	4	6	30-35	1	3		4	3		11
3-6	8	8	35-40	1	5		1	14		21
6-9	1	3	40-45	1	9	1	2	6	1	20
9 months and 1 year	2	8	45-50		10	1	2	1	3	17
1-2 years old	5	12	50-55	1	3	1	3	5	2	15
2-3	6	5	55-60		3	1		6	3	13
3-4	4	6	60-65	2	4	2	2	5	2	17
4-5	1	5	65-70		4	1	1	2	5	13
5-10	3	7	70-75	1	5	3		5	10	24
10-15	2	2	75-80			2	2	1	7	12
15-20	7	4	80			2	1		2	5
Total of the above ages,	60	75	81	1						1
			82		1	1			1	3
			83						2	2
			84					1	1	2
			85			1				1
			86						1	1
			87		1		1		2	4
			88					1		1
			90						1	1
			92		1				1	2
			97			1				1
			98					1	1	2
			106	1						1

Total of ages and conditions,

20	53	17	24	58	45	217
----	----	----	----	----	----	-----

Total of ages under 20 years, { Males, 60 } 135  
 { Females, 75 }

Total, 352

TABLE





## DISEASES.

## TABLE II.

58

Diseases of different Ages.

I. PERIL DISEASES.	Under 1 year.	Between 1 & 2.	3-4.	5-9.	10-14.	15-19.	20-29.	30-39.	40-49.	50-59.	60-69.	70-79.	80-89.	90- 100.	Total new.
Fever (Cullen's General) 56	1		1	5	3										33
Impetigo (G. 7)															1
Angina pectoris (G. 7)															1
Mortification (G. 7)															1
Quincy (G. 10.)	x														1
Infl. of the bowels (G. 10.)															1
Grievel and Stone (G. 19.)															1
Teething (Sauv. G. 198.)	1	1													2
Small-Pox (G. 20.)				1											1
Thrush (G. 33.)	1														1
Croupion (G. 35.)	2	1	3	1	2	3	9	15	14	16	7				73
Hæmorrhage (G. 37.)							x								1
II. NERVOUS DISEASES.															
Sudden death (G. 40 p)								x	1	2					7
Pally (G. 41.)									1	1	2				8
Sween p (G. 42.)									1	1					1
Indigestion (G. 43.)									1						5
Convulsions (G. 48. 50.)	50	8	3	3			x	2	x	3					69
Asthma (G. 52.)								2	x						16
Chinkough (G. 53.)	3	3	2	1	1			x	1	4					10
Colic (G. 55.)					1										3
Looferick (G. 57.)	1								1						1
Infancy (G. 63.)							x		x						3
III. DISEASES OF THE FLUIDS.															
Weakness of infancy (G. 65.)	4	4	2	3											13
Droopy of age (G. 66.)															1
Droopy of the brain (G. 72.)															1
Jandice (G. 87.)															2
IV. LOCAL DISEASES.															
Cancer (G. 114.)					1										2
Unknown diseases,	1						x	2	x						2
Catarrhes.															
Totals,	65	17	11	16	10	4	11	27	32	37	28	30	37	20	352

**MICROBIOLOGEN ENA.**  
C. 5-6. Titheneus. Synchysis.  
C. 7. Apollonia. medullant.  
C. 7. Canarica.  
C. 8. Eucorinth.  
C. 9. Nephritis.  
Saur. C. 198. Olantheia dentilipensis.  
C. 20. Carolia.  
**VOL. LXV.**

N

[illegible]

T A B L E III. Diseases of different Months.

DISEASES	Jan.	Feb.	March	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Total.
I. FEBRILE DISEASES.													
Fever (Cullen's Genera 5, 6.)	1	4	6	4		5	1	2		3	1	6	33
Imposthume (G. 7.)												1	1
Angina pectoris (G. 7.)						1							1
Mortification (G. 7.)		1		1		1	1			1	1	2	8
Quincy (G. 10.)				1									1
Infl. of the bowels (G. 16.)				1	1	1						1	4
Gravel and stone (G. 19.)				1	1								1
Teething (Sauv. 198)	1					1				1			2
Small-pox (G. 26.)													
Thrush (G. 38.)					1								1
Consumption (G. 35.)	5	11	8	6	10	5	5	3	1	1	8	10	73
Hæmorrhage (G. 37.)		1											1
II. NERVOUS DISEASES.													
Sudden death (G. 40.)						1		1		2		3	7
Palsy (G. 41.)	1	1	2	2							2		8
Swoon (G. 42.)			1										1
Indigestion (G. 43.)			1	2				1	1				5
Convulsions (G. 48, 50.)	8	7	3	11	6	7	4	6	3	2	3	9	69
Asthma (G. 52.)	2	3	5	2					2			2	16
Chinkcough (G. 53.)	4	3		1				1		1			10
Colic (G. 55.)		1											3
Looseness (G. 57.)											2		2
Infantry (G. 63.)			1	1	1						1		4
III. DISEASES OF THE FLANK.													
Weakness of infancy (G. 65.)		2	1	1	2			1				3	7
Decay of age (G. 66.)	3	10	7	8	8	5	2		3	4	6	6	62
Dropsy (G. 71, 75.)	2		1	1	2	1		2	1		2	1	13
Dropsy of the brain (G. 72.)			1										1
Jaundice (G. 87.)									1	1			2
IV. LOCAL DISEASES.													
Cancer (G. 114.)	1									1			2
Unknown diseases.					1						1		2
Casualties.													
Total,	28	45	37	42	33	33	14	16	12	18	27	47	352

GENERAL BILL OF MARRIAGES, BAPTISMS, and BURIALS.

For the YEAR 1773.

Marriages, 133. Baptisms, { Males, 216 } 402. Deaths, { Males, 150 } 352.  
 Females, 186 } 402. Females, 202 }







IX. *Experiments on a new Colouring Substance from the Island of Amsterdam in the South Sea. Made by Mr. Peter Woulfe, F. R. S. at the Desire of Sir John Pringle, Bart. P. R. S.*

Redde, Feb. 2, 1775. **T**HIS substance is of a light bright orange colour; has a peculiar, though not a strong, smell; and, when handled, gives a yellow stain to the skin, which does not readily wash out with soap and water. Put on a red hot iron, it smoaks, melts, and catches fire, leaving a *caput mortuum*. When boiled with water, it gives the liquor only a slight yellow tinge, which is but little heightened by the addition of a fixed alkaly; therefore the colouring part of this substance is insoluble in water. Oil of vitriol put to it becomes of a red orange colour; but, when the acid is drained off, the *residuum* appears purple. Annotto, treated in the same manner, gives a blue colour. Spirit of wine, æther, fixed and volatile alcalies, as also soap, dissolve the colouring part of this substance. To determine the quantity of colouring matter which it contains, two drams were digested in a matraass, with four ounces of rectified spirit of wine; the solution being filtered assumed a rich deep yellow colour, like a strong solution of saffron or gumbouge with the same spirit; what remained

Vol. LXV. O

mained in the filter was digested a second time, with four ounces of fresh spirit of wine, and the liquor filtered; this solution was much weaker than the first. The undissolved part remaining in the filter after this second solution was digested, a third time, with four ounces of fresh spirit; but the solution was now quite weak, and of a very pale yellow colour. The *residuum* being now deprived of its colouring portion, was slowly dried, when it appeared of a very pale yellow colour, felt as soft as starch between the fingers, and weighed forty two grains; so that two thirds nearly of this colouring substance are soluble in spirit of wine; the undissolved part is not soluble in water, acids or alkalies. Put on a red hot iron, it smokes and catches fire without melting, leaving a *caput mortuum*, and gives a smell similar to that arising from common vegetable matter. The first solution in spirit of wine, after standing twenty-four hours, deposits some of its colour in the form of minute spiculine crystals, of an orange colour. The second and third solutions let fall none of their colour. The first solution, dropped on paper, tinges it of a bright orange colour, the second gives a lively yellow colour, and the third a pale yellow. The first solution, sufficiently diluted with spirit of wine, makes a bright yellow stain on paper, no way inclining to an orange, but exactly resembling that made by the second solution; hence it seems probable, that an orange colour is only a deep yellow. Vitriolic æther readily dissolves the colouring part of this substance, and affords solutions of nearly the same colour as those made with

I

spirit

spirit of wine. Oil of turpentine dissolves but a small portion of it, and acquires only a pale yellow colour. A solution of fixed alkaly in water, digested with this substance, dissolves a large portion of its colouring part, and the solution is of a brownish yellow colour. Volatile spirit of *sal ammoniac*. seems to dissolve a larger portion of it than the fixed alkaly, and the solution is of a reddish orange colour. A solution of soap in water, boiled with this substance, likewise dissolves its colouring part. All the foregoing solutions, except that in oil of turpentine, which was not tried, die silk, cloth, and linen, of various shades of yellow and orange; but these colours are discharged, by boiling the dyed substances for some time in soap and water. This colour can, therefore, be of use only in dying silk and wool, for which purpose we are already furnished with good dyes. Few colours go so far in dying as this new substance, and none dye so speedily, especially when soap and water are used as the solvent; for a dip or two will dye cloth or silk of a lively yellow colour, when put into the mixture whilst hot. Soap and water may be perhaps used with advantage, as the solvent for several other colours.

From the foregoing experiments it appears, that this colouring substance, upon which they have been made, is of the resinous kind, and has a good deal of affinity with *annotta*.

X. *Experiments and Observations on the Gymnotus Electricus, or Electrical Eel.* By Hugh Williamson, M.D.  
*Communicated by John Walsh, Esq. F. R. S.*

TO JOHN WALSH, ESQ.

SIR,

London Feb. 7, 1775.

Redde, Feb. 9,  
 1775.

**A**S the electrical eel has lately engaged the public attention, and yours in particular, I have taken the liberty of sending you some experiments which I made on that fish: they are the same that I had the pleasure of shewing you last winter, on my arrival from Pennsylvania. If you apprehend they may tend to cast any light on that curious part of natural history, or to gratify the curiosity of the public, be pleased to make any use of them you may think proper. Besides my own superficial acquaintance with the subject of electricity, of which I am very conscious, there are other circumstances that may help to apologize for the imperfect state in which these experiments appear. The eel being sickened by the change of climate, its owner refused to let us take it out of the water, for the purpose of making experiments, on reasonable terms; and there were many experiments which I could not  
 make

make on it in the water, to my own satisfaction. While I made these experiments, the eel was kept in a large vessel, supported by pieces of dry timber, about three feet above the floor. Perhaps it may deserve notice, that a small hole being bored in the vessel in which the eel was swimming, one person provoked the eel so as to receive a shock; another person at the same time, not in contact with him, but holding his finger in the stream that spouted from the vessel, received a shock also in that finger. From this and fundry other experiments, I am induced to believe, that the *gymnotus* has powers greatly superior to, or rather different from, those of the *torpedo*, which you have examined with so much attention. I have the honour to be, SIR,

Your most obedient

and very humble servant,

HUGH WILLIAMSON.

Philadelphia, Sept. 3, 1773.

SOME weeks ago, a sea-faring man brought to this city a large eel, that had been caught in the province of Guiana, a little to the westward of Surinam. It had the extraordinary power of communicating a painful sensation, like that of an electrical shock, to people who touched it, and of killing its prey at a distance. As I have not heard that any other eel of this kind has ever been carried to any of our continental colonies, or that  
any

any of them have been seen in Europe, I shall take the liberty, after I have given a short description of the fish, to relate such experiments as I made, or assisted in making, in hopes of discovering by what means it produced the effects I have mentioned. The eel was three feet seven inches long, and about two inches thick near the head. On a transient view, it resembled one of our common eels both in shape and colour; but its head was flat and its mouth wide, like that of a cat-fish, without teeth. A fin, which was above two inches broad, extended along its belly, from the point of its tail to within six inches of its head. This fin was almost an inch thick where it adhered to the body; the upper part of it was muscular, but of a very different texture from the muscular part of the body; the difference was obvious to the touch, for I had no opportunity of making any observations by dissecting the subject. It was a native of fresh water, and breathed at the interval of three or four minutes, by lifting its head to the surface.

#### EXPERIMENTS.

1. On touching the eel with one of my hands, I perceived such a sensation in the joints of my fingers as I received on touching a prime conductor or charged phial, when no circle was formed; or such as I have received, when a few sparks of the electric fluid have been conveyed through my fingers only.
2. On touching the eel more roughly, I perceived a similar effect in my wrist and elbow.
3. Touching the eel with

with an iron rod, twelve inches long, I perceived the like sensation in the joints of the thumb and fingers with which I held the metal.

4. While another person provoked the eel by touching it, I put my hand into the water at the distance of three feet, and felt such a sensation in the joints of my fingers as when I had touched the eel, but not so painful.

5. Some small fishes were thrown into the water where he was swimming; he killed them immediately, and swallowed them.

6. A cat-fish <sup>(a)</sup>, that was at least one inch and an half thick, was thrown into the water where the eel was swimming; he killed it also, and attempted to swallow it, but could not.

7. In order to discover whether the eel killed those fish by an emission of the same fluid with which he affected my hand when I had touched him, I put my hand into the water, at some distance from the eel; another cat-fish was thrown into the water; the eel swam up to it, but presently turned away, without offering any violence. After some time he returned; when, seeming to view it for a few seconds, he gave it a shock, by which it instantly turned up its belly, and continued motionless; at that very instant I felt such a sensation in the joints of my fingers as in experiment 4.

8. A third cat-fish was thrown into the water, to which the eel gave such a shock, that it turned on its side, but continued to give signs of life. The eel seeming to observe this, as it was turning away, immediately returned, and struck it quite motionless. I could easily perceive that the last

(a) The Bayre de rio of Maregrave.



shock was more severe than the former. The eel never attempted to swallow any of those fish after the first, though he killed many of them; and I always observed, that when he was going to kill one, he swam directly up to it, as if he was going to bite it; that when he came up, he sometimes paused before he gave the shock, at other times he gave the shock immediately. When we removed any of those cat-fish, though apparently dead, into water in another vessel, they presently recovered. Fish that are stunned by a small electrical shock were found to recover in the same manner.

9. Touching the eel, so as to provoke it, with one hand, and at the same time holding my other hand in the water, at a small distance, a shock passed through both my arms, as in the case of the Leyden experiment.

10. I put the end of a wet stick into the water, and holding it with one hand, I touched the eel with the other; a shock passed through both arms as before.

11. Taking another gentleman in company by the hand, he touched the eel, while I held one of my hands in the water; the shock passed through us both.

12. Instead of putting my hand into the water, at a distance from the eel, as in the last experiment, I touched its tail, so as not to offend it, while my assistant touched its head more roughly; we both received a severe shock.

13. Eight or ten persons, taking hands, stood in a circular form; the first in the series touched the eel, while the last put his hand into the water, at some distance from it; they all received a gentle shock.

14. The above experiment

ment was repeated with no other variation than that the last person touched the eel's tail, while the first touched its head; they all received a severe shock. 15. Another gentleman and myself, holding the extremities of a brass chain, one of us put his hand into the water, while the other touched the eel, so as to offend it; the shock passed through us both. 16. I wrapped a silk handkerchief round my hand, and touched the eel with it, but received no shock; although another gentleman felt the shock, who, at the same time, put his hand into the water, at some distance from the eel. 17. A great variety of other experiments were made by two persons, one touching the eel near its head, the other putting his hand into the water, or touching it near the tail, forming a communication at the same time between their hands, which were out of the water, by pieces of charcoal, rods of iron or brass, a piece of dry wood, glass, silk, &c. The uniform result of all those experiments was, that whatever uses to convey the electrical fluid would also convey the fluid discharged by the eel; and *vice versa*, a brass chain, that had very many links in it, would not convey it, unless when the shock was severe, or the chain tense. 18. One of the company being insulated on glass bottles, received several shocks from the eel; but he exhibited no marks of a *plus* state of electricity, nor would cork-balls, suspended by filken threads, give any marks of it, either when they were suspended over the eel's back, or touched by the insulated person at the instant he received the shock. 19.

A person, holding a phial in one hand properly lined and coated for electrical experiments, put his hand to the tail of the fish, while an assistant, holding a short wire in one hand that communicated with the inside of the phial, grasped the fish near its head, so as to receive a severe shock in his hand and arm, but it passed no further.

20. Two pieces of brass wire, about the thickness of a crow's quill, were screwed, in opposite directions, into a frame of wood, so as to come within less than the hundredth part of an inch of contact; they were rounded at the point. I held the remote end of one of those wires, while an assistant held the other; in the mean while, one of us putting his hand into the water near the eel, the other touched it so as to receive a shock. We repeated this experiment fifteen or twenty times with different success: when the points of the wires were even screwed asunder, to the fiftieth part of an inch, the shock never passed in the circle; but when they were screwed up within the thickness of double-post paper, the shocks, such of them as were severe, would pass through us both; in which case, they doubtless leaped from the point of one wire to the other, though we were not so fortunate as to render the spark generally visible. But it should be observed, that the eel on which we made these experiments, was not easily provoked, and appeared to be in bad health. I have frequently passed my hand along its back and sides from head to tail, and have lifted part of its body above the water, without tempting it to make any defence. Dr. BANCROFT tells us, that such eels in

Guiana

Guiana have shocked his hand at the distance of some inches from the surface of the water. Perhaps fire emitted by eels lately taken, might be rendered visible.

From the above experiments it appears: 1. That the Guiana eel has the power of communicating a painful sensation to animals that touch or come near it. 2. That this effect depends entirely on the will of the eel; that it has the power of giving a small shock, a severe one, or none at all, just as circumstances may require. 3. That the shock given, or the painful sensation communicated, depends not on the muscular action of the eel, since it shocks bodies in certain situations at a great distance; and since particular substances only will convey the shock, while others, equally elastic or hard, refuse to convey it. 4. That the shock must therefore depend upon some fluid, which the eel discharges from its body. 5. That as the fluid discharged by the eel affects the same parts of the human body that are affected by the electric fluid; as it excites sensations perfectly similar; as it kills or stuns animals in the same manner; as it is conveyed by the same bodies that convey the electric fluid, and refuses to be conveyed by other bodies that refuse to convey the electric fluid, it must also be the true electrical fluid; and the shock given by this eel must be the true electrical shock.

XI. *An Account of the Gymnotus Electricus, or Electrical Eel. In a Letter from Alexander Garden, M. D. F. R. S. to John Ellis, Esq. F. R. S.*

SIR,

Charles-Town, South Carolina,  
Aug. 14, 1774.

Reads, Feb. 23,  
1775.

A FEW days since, I went to see some very curious fish, which were brought here about nine or ten weeks ago from Surinam; and I was both surprized and delighted to observe their strange shape, and experience their wonderful properties. I had before received some vague account of such a fish; but I always thought, that much of what I heard was fabulous. There are five of these fishes now here, of different sizes, from two feet in length to three feet eight inches. The following description was made out from the longest and largest. It might have been much more accurate, if there had been a possibility of handling the fish, and examining it leisurely; or if I could have had a dead specimen, as many things relating to the internal and external structure could in that case have been more exactly ascertained. But this fish hath the amazing power of giving so sudden and so violent a shock to any person that touches it, that there is, I think, an absolute impossibility of ever examining accurately a living specimen,

specimen; and the person who owns them rates them at too high a price (not less than fifty guineas for the smallest) for me to get a dead specimen, unless one should die by accident; if that should happen, you may depend on having a more exact and accurate account for the Society.

GEORGE BAKER, mariner, who brought them here, intends to carry them to England; but as it is very uncertain whether they will arrive in health and all alive, I have recommended to him to get a small cask of rum, with a large bung, into which he may put any of them that may die, and so preserve them for the inspection and examination of the curious when he arrives.

The largest of these fish was three feet eight inches in length, when extending itself most, and might have been from ten to fourteen inches in circumference about the thickest part of his body. The head is large, broad, flat, smooth, and impressed here and there with holes, as if perforated with a blunt needle, especially towards the sides, where they are more regularly ranged in a line on each side. The *rostrum* is obtuse and rounded. The upper and lower jaws are of an equal length, and the gape is large. The nostrils are two on each side; the first large, tubular, and elevated above the surface; and the others small, and level with the skin, placed immediately behind the verge of the *rostrum*, at the distance of an inch asunder. The eyes are small, flattish, and of a blueish colour, placed about three quarters of an inch behind the nostrils, and more towards the sides of the head. The whole head seems to be well supported;  
but

but whether with bones or cartilages, I could not learn. The body is large, thick, and roundish, for a considerable distance from the head, and then gradually grows smaller, but at the same time deeper, or becomes of an *acinaciform* shape, to the point of the tail, which is rather blunt. There are many light-coloured spots on the back and sides of the body, placed at considerable distances in irregular lines, but more numerous and distinct towards the tail. When the fish was swimming, it measured six inches in depth near the middle, from the upper part of the back to the lower edge of the fin, and it could not be more than two inches broad on the back at that place. The whole body, from about four inches below the head, seems to be clearly distinguished into four different longitudinal parts or divisions. The upper part or back is roundish, of a dark colour, and separated from the other parts on each side by the *lateral lines*; which, taking their rise at the base of the head, just above the pectoral fins, run down the sides, gradually converging, as the fish grows smaller, to the tail, and make so visible a depression or furrow in their course, as to distinguish this from the second part or division, which may be properly called the body, or at least, appears to be the strong muscular part of the fish. This second division is of a lighter and more clear blueish colour than the upper or back part, and seems to swell out somewhat on each side, from the depression of the lateral lines; but, towards the lower or under part, is again contracted, or sharpened into the third part, or *carina*. This *carina*, or keel, is very distinguishable

tinguishable from the other two divisions, by its thinness, its apparent laxness, and by the reticulated skin of a more grey and light colour, with which it is covered. When the animal swims gently in pretty deep water, the rhomboidal reticulations of the skin of this *carina* are very discernible; but when the water is shallow, or the depth of the *carina* is contracted, these reticulations appear like many irregular longitudinal *plicæ*. The *carina* begins about six or seven inches below the base of the head, and gradually widening or deepening as it goes along, reaches down to the tail, where it is thinnest. It seems to be of a strong muscular nature. Where it first takes its rise from the body of the fish, it seems to be about one inch or one inch and an half thick, and is gradually sharpened to a thin edge, where the fourth and last part is situated; *videlicet*, a long, deep, soft, wavy fin, which takes its rise about three or four inches at most below the head, and runs down along the sharp edge of the *carina* to the extremity of the tail. Where it first rises it is not deep, but gradually deepens or widens as it approaches to the tail. It is of a very pliable soft consistence, and seems rather longer than the body. The situation of the *anus* in this fish is very singular, being placed underneath, and being about an inch more forward than the pectoral fins, and consequently considerably nearer the *rostrum*. It is a pretty long *rima* in appearance; but the aperture must be very small, as the formed excrements are only about the size of a quill of a common dunghill fowl. There are two pectoral (if I



may call them so) fins, placed one on each side, just behind the head, over the *foramina spiratoria*, which are small, and generally covered with a lax skin, situated in the *axille* of these fins. These fins are small for the size of the fish, being scarcely an inch in length, of a very thin, delicate consistence, and orbicular shape. They seem to be chiefly useful in supporting and raising the head of the fish when he wants to breathe, which he does every four or five minutes, by raising his mouth out of the water. This shews that he has lungs and is amphibious, and the *foramina spiratoria* seem to indicate his having *branchiæ* likewise; but this I only offer as a conjecture, not being certain of the fact. I must now mention the appearances of a number of small cross bands, annular divisions, or rather *rugæ* of the skin of the body. They reach across the body down to the base of the *carina* on each side; but those that cross the back seem to terminate at the lateral lines, where new rings take their rise, not exactly in the same line, and run down to the *carina*. This gives the fish somewhat of a worm-like appearance; and indeed it seems to have some of the properties of this tribe, for it has a power of lengthening or shortening its body to a certain degree, for its own conveniency, or agreeable to its own inclination. I have seen this specimen, which I have measured three feet eight inches, shorten himself to three feet two inches; but besides this power of lengthening or shortening his body, he can swim forwards or backwards with apparently equal ease to himself, which is another property

perty of the vermicular tribe. When he swims forward, the undulation or wavy motion of the fin and *carina* begin from the upper part, and move downwards; but when he swims backwards, and the tail goes foremost, the undulations of the fin begin at the extremity of the tail or fin, and proceed in succession from that backwards to the upper part of the body; in either case he swims equally swift. Every now and then the fish lays himself on one side, as it were, to rest himself, and then the four several divisions of his body abovementioned are very distinctly seen; *videlicet*, the vermiform appearance of the two upper divisions; the retiform appearance of the *carina*; and the last, or dark-coloured fin, whose rays seem to be exceedingly soft and flexible, and entirely at the command of the strong muscular *carina*. When he is taken out of the water, and laid on his belly, the *carina* and fin lie to one side, in the same manner as the ventral fin of the *Tetraodon* does, when he creeps on the ground. I have been the longer and more particular in the description of the external structure of this animal's body, because I think, as it is of a most singular nature, and endowed with some amazing properties, even the most minute circumstance I was able to observe relating to it should be mentioned.

The person to whom these animals belong, calls them *Electrical Fish*; and indeed the power they have of giving an electrical shock to any person, or to any number of persons who join hands together, the extreme person on each side touching the fish, is their most singular and

astounding property. All the five we have here are possessed of this power in a very great degree, and communicate the shock to one person, or to any number of persons, either by the immediate touch of the fish with the hand, or by the mediation of any metalline rod. The keeper says, that when they were first caught, they could give a much stronger shock by a metalline conductor than they can do at present. The person who is to receive the shock must take the fish with both hands, at some considerable distance asunder, so as to form the communication, otherwise he will not receive it; at least I never saw any one shocked from taking hold of it with one hand only: though some have assured me, that they were shocked by laying one hand on him. I myself have taken hold of the largest with one hand often, without ever receiving a shock; but I never touched it with both hands, at a little distance asunder, without feeling a smart shock. I have often remarked, that when it is taken hold of with one hand, and the other hand is put into the water over its body, without touching it, the person received a smart shock; and I have observed the same effect follow, when a number joined hands, and the person at one extremity of the circle took hold of, or touched the fish, and the person at the other extremity put his hand into the water, over the body of the fish. The shock was communicated through the whole circle, as smartly as if both the extreme persons had touched the fish. In this it seems to differ widely from the *torpedo*, or else we are much misinformed of the manner in

in which the benumbing effect of that fish is communicated. The shock which our Surinam fish gives, seems to be wholly electrical; and all the phænomena or properties of it exactly resemble those of the electric *aura* of our atmosphere when collected, as far as they are discoverable from the several trials made on this fish. This stroke is communicated by the same conductors, and intercepted by the interposition of the same original electrics, or electrics *per se* as they used to be called. The keeper of this fish informs me, that he caught them in Surinam river, a great way up, beyond where the salt water reaches; and that they are a fresh water fish only. He says, that they are eaten, and by some people esteemed a great delicacy. They live on fish, worms, or any animal food, if it is cut small, so that they can swallow it. When small live fishes are thrown into the water, they first give them a shock, which kills or so stupifies them, that they can swallow them easily, and without any trouble. If one of these small fishes, after it is shocked, and to all appearance dead, be taken out of the vessel where the electrical fish is, and put into fresh water, it will soon revive again. If a larger fish than they can swallow be thrown into the water, at a time that they are hungry, they give him some smart shocks, till he is apparently dead, and then they try to swallow or suck him in; but, after several attempts, finding he is too large, they quit him. Upon the most careful inspection of such fish, I could never see any mark of teeth, or the least wound or scratch on them. When the electrical fish

are hungry, they are pretty keen after their food; but they are soon satisfied, not being able to contain much at one time. An electrical fish of three feet and upwards in length cannot swallow a small fish above three or at most three inches and a half long. Since I wrote the above description and remarks, I have had Mr. BANCROFT's Essay on the Natural History of Guiana put into my hands, in which I find an account of this animal; but, as I think that he has not been very particular in the description of it, I resolved still to send you the above account, that you might judge for yourself. I observe, that his account or description and mine differ in several things; and amongst others, where he says, that those fish were usually about three feet in length; but the one, of which I have sent a slight description, was three feet eight inches. This small variation might indeed have happened without any error; but I am told, that some of them have been seen in Surinam river upwards of twenty feet long, whose stroke or shock proved instant death to any person that unluckily received it.

I shall be on the watch to procure a more accurate knowledge of, and acquaintance with, this animal; and if I can learn any thing farther about it, you may depend on my communicating it.

XII. *Experiments and Observations in an heated Room.*

By Charles Blagden, M. D. F. R. S.

Redde, Feb. 16, 1774. **A**BOUT the middle of January, several gentlemen and myself received an invitation from Dr. GEORGE FORDYCE, to observe the effects of air heated to a much higher degree than it was formerly thought any living creature could bear. We all rejoiced at the opportunity of being convinced, by our own experience, of the wonderful power with which the animal body is endued, of resisting an heat vastly greater than its own temperature; and our curiosity was not a little excited to observe the circumstances attending this remarkable power. We knew, indeed, that of late several convincing arguments had been adduced, and observations made, to shew the error of the common opinions on this subject; and that Dr. FORDYCE had himself proved the mistake of Dr. BOERHAAVE<sup>(a)</sup> and most other authors, by supporting many times very high degrees of heat, in the course of a long train of important experiments; with which, and his most philosophical conclusions from them, every lover of science must earnestly wish that he may soon favour the public. In the mean time time, I am happy in an opportunity of laying before this So-

(a) Elem. Chæmizæ, tom. I. p. 277, 278.

ciety the following short account of some of these experiments, and of the views with which they were undertaken; for the particulars of which I am obliged to Dr. FORDYCE himself.

DR. CULLEN long ago suggested many arguments to shew, that life itself had a power of generating heat, independent of any common chemical or mechanical means; for, before his time, the received opinions were, that the heat of animals arose either from friction or fermentation<sup>(b)</sup>. Governor ELLIS in the year 1758 observed<sup>(c)</sup>, that a man can live in air of a greater heat than that of his body; and that the body, in this situation, continues its own cold. The Abbé CHAPPE D'AUTEROCHE informs us, that the Russians use their baths heated to 60°<sup>(d)</sup> of REAUMUR's thermometer, about 160 of FAHRENHEIT's, without taking notice, however, of the heat of their bodies when bathing. With a view to add further evidence to these extraordinary facts, and to ascertain the real effects of such great degrees of heat on

(b) To do further justice to the philosophy of this most ingenious and respectable professor, I must here declare, that during my stay in Edinburgh, from the year 1765 to 1769, the idea of a power in animals of *generating cold* (that was the expression) when the heat of the atmosphere exceeded the proper temperature of their bodies, was pretty generally received among the students of physic, from Dr. CULLEN's arguments; in consequence of which I applied a thermometer, in a hot summer day, to the belly of a frog, and found the quicksilver sink several degrees: a rude experiment indeed, but serving to confirm the general fact, that the living body possesses a power of resisting the communication of heat.

(c) Philosophical Transactions, vol. L. p. 755.

(d) Voy. en Sibirie, tom. I. p. 51.

the human body, Dr. FORDYCE tried the following experiments.

He procured a *suite* of rooms, of which the hottest was heated by flues in the floor, and by pouring upon it boiling water; and the second was heated by the same flues, which passed through its floor to the third. The first room was nearly circular, about ten or twelve feet in diameter and height, and covered with a dome, in the top of which was a small window. The second and third rooms were square, and both furnished with a sky-light. There was no chimney in these rooms, nor any vent for the air, excepting through crevices at the door. In the first room were placed three thermometers; one in the hottest part of it, another in the coolest part, and a third on the table, to be used occasionally in the course of the experiment: the frame of this last was made to turn back by a joint, so as to leave the ball and about two inches of the stem quite bare, that it might be more conveniently applied for ascertaining the heat of the body, and several other purposes.

#### EXPERIMENT I.

In the first room the highest thermometer stood at  $120^{\circ}$ , the lowest at  $110^{\circ}$ ; in the second room the heat was from  $90^{\circ}$  to  $85^{\circ}$ ; the third room felt moderately warm, while the external air was below the freezing point. About three hours after breakfast, Dr. FORDYCE having taken off all his cloaths, except his shirt, in the third room, and being furnished with wooden shoes, or rather fandals tied on with liss, entered into the second room, and staid five minutes



minutes in a heat of  $90^{\circ}$ , when he began to sweat gently. He then entered the first room, and stood in the part heated to  $110^{\circ}$ ; in about half a minute his shirt became so wet that he was obliged to throw it aside, and then the water poured down in streams over his whole body. Having remained ten minutes in this heat of  $110^{\circ}$ , he removed to the part of the room heated to  $120^{\circ}$ ; and after staying there twenty minutes, he found that the thermometer placed under his tongue, and held in his hand, stood just at  $100^{\circ}$ , and that his urine was of the same temperature. His pulse had gradually risen till it made 145 pulsations in a minute. The external circulation was greatly increased; the veins had become very large, and an universal redness had diffused itself over the body, attended with a strong feeling of heat. His respiration, however, was but little affected. Here Dr. FORDYCE remarks, that the moisture of his skin most probably proceeded chiefly from the condensation of the vapour in the room upon his body. He concluded this experiment in the second room, by plunging into water heated to  $100^{\circ}$ ; and, after having been wiped dry, was carried home in a chair; but the circulation did not subside for two hours, after which he walked out in the open air, and scarcely felt the cold.

#### EXPERIMENT II.

In the first room the highest thermometer varied from  $132^{\circ}$  to  $130^{\circ}$ ; the lowest stood at  $119^{\circ}$ . Dr. FORDYCE having undressed in an adjoining cold chamber, went into the heat of  $119^{\circ}$ ; in half a minute the water poured down in streams over his whole body, so as to keep that part of the floor

floor where he stood constantly wet. Having remained here fifteen minutes, he went into the heat of  $130^{\circ}$ ; at this time the heat of his body was  $100^{\circ}$ , and his pulse beat 126 times in a minute. While Dr. FORDYCE stood in this situation, a Florence flask was brought in, by his order, filled with water heated to  $100^{\circ}$ , and a dry cloth, with which he wiped the surface of the flask quite dry; but it immediately became wet again, and streams of water poured down its sides; which continued till the heat of the water within had risen to  $122^{\circ}$ , when Dr. FORDYCE went out of the room, after having remained fifteen minutes in an heat of  $130^{\circ}$ ; just before he left the room his pulse made 139 beats in a minute, but the heat under his tongue, in his hand, and of his urine, did not exceed  $100^{\circ}$ . Here Dr. FORDYCE observes, that as there was no evaporation, but constantly a condensation of vapour on his body, no cold was generated but by the animal powers. At the conclusion of this experiment, Dr. FORDYCE went into a room where the thermometer stood at  $43^{\circ}$ , dressed himself there, and immediately went out into the cold air, without feeling the least inconvenience; on which he remarks, that the transition from very great heat to cold is not so hurtful as might be expected, because the external circulation is so excited, as not to be readily overcome by the cold. Dr. FORDYCE has since had occasion, in making other experiments, to go frequently into a much greater heat, where the air was dry, and to stay there a much longer time, without being affected nearly so much, for which he assigns two reasons;

that dry air does not communicate its heat like air saturated with moisture; and that the evaporation from the body, which takes place when the air is dry, assists its living powers in producing cold. It must be immediately perceived, that, besides the principal object, these curious experiments throw great light on many other very important subjects of natural philosophy.

January 23. The honourable Captain PHIPPS, Mr. BANKS, Dr. SOLANDER, and myself, attended Dr. FORDYCE to the heated chamber, which had served for many of his experiments with dry air. We went in without taking off any of our cloaths. It was an oblong-square room, fourteen feet by twelve in length and width, and eleven in height, heated by a round stove, or *cockle*, of cast iron, which stood in the middle, with a tube for the smoke carried from it through one of the side walls. When we first entered the room, about 2 o'clock in the afternoon, the quicksilver in a thermometer which had been suspended there stood above the 150th degree. By placing several thermometers in different parts of the room we afterwards found, that the heat was a little greater in some places than in others; but that the whole difference never exceeded 20°. We continued in the room above 20 minutes, in which time the heat had risen about 12°, chiefly during the first part of our stay. Within an hour afterwards we went into this room again, without feeling any material difference, though the heat was considerably increased. Upon entering the room a third time, between five and six o'clock after dinner, we observed

served the quicksilver in our only remaining thermometer at  $198^{\circ}$ <sup>(e)</sup>: this great heat had so warped the ivory frames of our other thermometers that every one of them was broken. We now staid in the room, all together, about 10 minutes; but finding that the thermometer sunk very fast, it was agreed, that for the future only one person should go in at a time, and orders were given to raise the fire as much as possible. Soon afterwards Dr. SOLANDER entered the room alone, and saw the thermometer at  $210^{\circ}$ ; but, during three minutes that he staid there, it sunk to  $196^{\circ}$ . Another time, he found it almost five minutes before the heat was lessened from  $210^{\circ}$  to  $196^{\circ}$ . Mr. BANKS closed the whole, by going in when the thermometer stood above  $211^{\circ}$ ; he remained seven minutes, in which time the quicksilver had sunk to  $198^{\circ}$ ; but cold air had been let into the room, by a person who went in and came out again during Mr. BANKS's stay. The air heated to these high degrees felt unpleasantly hot, but was very bearable. Our most uneasy feeling was a sense of scorching on the face and legs; our legs particularly suffered very much, by being exposed more fully than any other part to the body of the stove, heated red-hot by the fire within. Our respiration was not at all affected; it became neither quick nor laborious; the only difference was a want of that refreshing sensation which accompanies a full inspiration of cool air. Our time was so taken up with other observations that we did not

(e) This thermometer stands, near the boiling point, about a degree too high; the scale is FAHRENHEIT'S.

count our pulses by the watch: mine, to the best of my judgment by feeling it, beat at the rate of 100 pulsations in a minute, near the end of the first experiment; and Dr. SOLANDER's made 92 pulsations in a minute soon after we had gone out of the heated room. Mr. BANKS sweated profusely, but no one else; my shirt was only damp at the end of the experiment. But the most striking effects proceeded from our power of preserving our natural temperature. Being now in a situation in which our bodies bore a very different relation to the surrounding atmosphere from that to which we had been accustomed, every moment presented a new phenomenon. Whenever we breathed on a thermometer the quicksilver sunk several degrees. Every expiration, particularly if made with any degree of violence, gave a very pleasant impression of coolness to our nostrils, scorched just before by the hot air rushing against them when we inspired. In the same manner our now cold breath agreeably cooled our fingers whenever it reached them. Upon touching my side, it felt cold like a corpse; and yet the actual heat of my body, tried under my tongue, and by applying closely the thermometer to my skin, was  $98^{\circ}$ , about a degree higher than its ordinary temperature. When the heat of the air began to approach the highest degree which this apparatus was capable of producing, our bodies in the room prevented it from rising any higher; and when it had been previously raised above that point, inevitably sunk it. Every experiment furnished proofs of this: toward the end of the first, the thermometer

meter was stationary: in the second, it sunk a little during the short time we staid in the room: in the third, it sunk so fast as to oblige us to determine that only one person should go in at a time: and Mr. BANKS and Dr. SOLANDER each found, that his single body was sufficient to sink the quicksilver very fast, when the room was brought nearly to its *maximum* of heat.

These experiments, therefore, prove in the clearest manner, that the body has a power of destroying heat. To speak justly on this subject, we must call it a power of destroying a certain degree of heat communicated with a certain quickness. Therefore in estimating the heat which we are capable of resisting, it is necessary to take into consideration not only what degree of heat would be communicated to our bodies, if they possessed no resisting power, by the heated body, before the equilibrium of heat was effected; but also what time that heat would take in passing from the heated body into our bodies. In consequence of this compound limitation of our resisting power, we bear very different degrees of heat in different mediums. The same person who felt no inconvenience from air heated to  $211^{\circ}$ , could not bear quicksilver at  $120^{\circ}$ , and could just bear rectified spirit of wine at  $130^{\circ}$ ; that is, quicksilver heated to  $120^{\circ}$  furnished, in a given time, more heat for the living powers to destroy, than spirits heated to  $130^{\circ}$ , or air to  $211^{\circ}$  (f).

And

(f) These numbers are the result of some experiments which were made on the first of February, in a room where the heat of the air was  $65^{\circ}$ . Mr.

BANKS

And we had in the heated room where our experiments were made, a striking though familiar instance of the same. All the pieces of metal there, even our watch-chains, felt so hot, that we could scarcely bear to touch them for a moment, whilst the air, from which the metal had derived all its heat, was only unpleasant. The slowness with which air communicates its heat was further shewn, in a remarkable manner, by the thermometers we brought with us into the room, none of which at the end of twenty minutes, in the first experiment, had acquired the real heat of the air by several degrees. It might be supposed, that by an action so very different from that to which we are accustomed, as destroying a large quantity of heat, instead of generating it, we must have been greatly disordered. And indeed we experienced some inconvenience; our hands shook very much, and we felt a considerable degree of languor and debility; I had also a noise and giddiness in my head. But it was only a small part of our bodies that exerted the power of destroying heat with such a violent effort as seems necessary at first sight. Our cloaths, contrived to guard us from cold, guarded us from the heat on the same principles. Underneath we were surrounded with an atmo-

**BANKS** and I found that we could bear spirits which had been considerably heated and were now cooling, when the thermometer came to the 130th degree; cooling oil at 120°; cooling water at 123°; cooling quicksilver at 117°. And these points were pretty nicely determined; so that though we could bear water very well at 123°, we could not bear it at 125°, an experiment in which **Dr. SOLANDER** joined us. And our feelings with respect to all these points, seemed pretty exactly the same.

sphere of air, cooled on one side to  $98^{\circ}$ , by being in contact with our bodies, and on the other side heated very slowly, because woollen is such a bad conductor of heat. Accordingly I found, toward the end of the first experiment, that a thermometer put under my cloaths, but not in contact with my skin, sunk down to  $110^{\circ}$ . On this principle it was that the animals, subjected by M. TILLET to the interesting experiments related in the Memoirs of the Academy of Sciences for the year 1764, bore the oven so much better when they were cloathed, than when they were put in bare: the heat actually applied to the greatest part of their bodies was considerably less in the first case than in the last. As animals can destroy only a certain quantity of heat in a given time, so the time they can continue the full exertion of this destroying power seems to be also limited; which may be one reason why we can bear for a certain time, and much longer than can be necessary to fully heat the *cuticle*, a degree of heat which will at length prove intolerable. Probably both the power of destroying heat, and the time for which it can be exerted, may be increased, like most other faculties of the body, by frequent exercise. It might be partly on this principle that, in M. TILLET's experiments, the girls who had been used to attend the oven bore, for ten minutes, an heat which would raise FAHRENHEIT's thermometer to  $280^{\circ}$ : in our experiments, however, not one of us thought he suffered the greatest degree of heat that he was able to support.

A principal



A principal use of all these facts is, to explode the common theories of the generation of heat in animals. No attrition, no fermentation, or whatever else the mechanical and chemical physicians have devised, can explain a power capable of producing or destroying heat, just as the circumstances of the situation require. A power of such a nature, that it can only be referred to the principle of life itself, and probably exercised only in those parts of our bodies in which life seems peculiarly to reside. From these, with which no considerable portion of the animal body is left unprovided, the generated heat may be readily communicated to every particle of inanimate matter that enters into our composition. This power of generating heat seems to attend life very universally. Not to mention other well known experiments, Mr. HUNTER found a carp preserve a coat of fluid water round him, long after all the rest of the water in the vessel had been congealed by a very strong freezing mixture. And as for insects, Dr. MARTINE <sup>(s)</sup> observed, that his thermometer, buried in the midst of a swarm of bees, rose to 97°. It seems extremely probable, that vegetables, together with the many other vital powers which they possess in common with animals, have something of this property of generating heat. I doubt, if the sudden melting of snow which falls upon grass, whilst that on the adjoining gravel walk continues so many hours unthawed, can be adequately explained on any other supposition. Moist dead

(s) *Essays Medical and Philosophical*, p. 331.

sticks are often found frozen quite hard, when in the same garden the tender growing twigs are not at all affected. And many herbaceous vegetables, of no great size, resist every winter degrees of cold which are found sufficient to freeze large bodies of water. It may be proper to add, that after each of the above mentioned experiments of bearing high degrees of heat, we went out immediately into the open air, without any precaution, and experienced from it no bad effect; The languor and shaking of our hands soon went off, and we have not since suffered the least inconvenience.

XIII. *The supposed Effect of boiling upon Water, in disposing it to freeze more readily, ascertained by Experiments.*

By Joseph Black, M. D. Professor of Chemistry at Edinburgh, in a Letter to Sir John Pringle, Bart. P. R. S.

TO SIR JOHN PRINGLE, BART. P. R. S.

DEAR SIR,

Edinburgh, Feb. 11, 1775.

Redde, Feb. 23,  
1775.

WE had lately one day of a calm and clear frost; and I immediately seized the opportunity, which I missed before, to make some experiments relative to the freezing of boiled water, in comparison with that of water not boiled. I ordered some water to be boiled in the tea kettle four hours. I then filled with it a Florentine flask, and immediately applied snow to the flask until I cooled it to  $48^{\circ}$  of FAHRENHEIT, the temperature of some unboiled water which stood in my study in a bottle; then putting four ounces of boiled, and four of the unboiled water, separately, into two equal tea cups, I exposed them on the outside of a north window, where a thermometer pointed to  $29^{\circ}$ . The consequence was, that ice appeared first upon the boiled water; and this, in several repetitions of the experiment, with the same boiled water, some of which

which were made nine hours after it was poured out of the tea kettle. The length of time which intervened between the first appearance of ice upon the two waters was different in the different experiments. One cause of this variety was plainly a variation of the temperature of the air, which became colder in the afternoon, and made the thermometer descend gradually to  $25^{\circ}$ . Another cause was the disturbance of the water; when the unboiled water was disturbed now and then by stirring it gently with a quill tooth-pick, the ice was formed upon it as soon, or very nearly as soon, as upon the other; and from what I saw, I have reason to think, that were it to be stirred incessantly, provided at the same time the experiment were made with quantities of water, not much larger or deeper than these, it would begin to freeze full as soon. In one of these trials, having inspected my tea cups when they had been an hour exposed, and finding ice upon the boiled water, and none upon the other, I gently stirred the unboiled water with my tooth-pick, and saw immediately, under my eye, fine feathers of ice formed on its surface, which quickly encreased in size and number, until there was as much ice in this cup as in the other, and all of it formed in one minute of time, or two at most. And in the rest of the trials, though the congelation began in general later in the unboiled water than in the other; when it did begin in the former, the ice quickly encreased so as, in a very short time, to equal, or nearly equal in quantity, that which had been formed more gradually in the boiled water. The opinion, therefore,

therefore, which I have formed from what I have hitherto seen is, that the boiled and common water differ from one another in this respect; that whereas the common water, when exposed in a state of tranquillity to air that is a few degrees colder than the freezing point, may easily be cooled to the degree of such air, and still continue perfectly fluid, provided it still remain undisturbed: the boiled water, on the contrary, cannot be preserved fluid in these circumstances; but when cooled down to the freezing point, if we attempt to make it in the least colder, a part of it is immediately changed into ice; after which, by the continued action of the cold air upon it, more ice is formed in it every moment, until the whole of it be gradually congealed before it can become as cold as the air that surrounds it. From this discovery it is easy to understand, why they find it necessary to boil the water in India, in order to obtain ice. The utmost intensity of the cold which they can obtain by all the means they employ, is probably not greater than  $31^{\circ}$  or  $30^{\circ}$  of FAHRENHEIT'S thermometer. Common water, left undisturbed, will easily descend to this degree without freezing; and, if they have not the means of making it colder, may continue fluid for any time, provided it be not disturbed: the refrigerating causes of that part of the world when they have done so much, have done their utmost, and can act no further upon the water. But this cannot happen to the boiled water; when the refrigerating causes have cooled it to  $32^{\circ}$ , the next effect they produce, is to occasion in it the beginning of congelation, while  
the

the water is afterwards gradually assuming the form of ice, we know, by experience, that the temperature of it must remain at  $32^{\circ}$ ; it cannot be made colder, so long as any considerable part of it remains unfrozen<sup>(a)</sup>. The refrigerating causes continue, therefore, to have power over it, and to act upon it, and will gradually change the whole into ice, if their action be continued sufficiently long.

The next object of investigation may be the cause of this difference between the boiled and the common water. In considering this point, the following idea was suggested. As we know from experience, that by disturbing common water, we hasten the beginning of its congelation, or render it incapable of being cooled below  $32^{\circ}$ , without being congealed; may not the only difference between it and boiling water, when they are exposed together to a calm frosty air, consist in this circumstance: that the boiled water is necessarily subjected to the action of a disturbing cause, during the whole time of its exposure, which the other is not? One effect of boiling water long, is to expell the air which it naturally contains; as soon as it cools, it begins to attract and absorb air again, until it hath recovered its former quantity; but this probably requires a considerable time. During the whole of this time, the air entering into it must occasion an agitation or disturbance in the water, which, though not sensible to the eye, may be very effectual in

(a) Common water, when cooled in a state of tranquillity to several degrees below the freeing point, will suddenly rise up to it again, if disturbed in such a manner as to occasion in it a beginning of congelation.

preventing it to become, in the least, colder than the freezing point, without beginning to freeze, in consequence of which its congelation must begin immediately after it is cooled to that point. When I reflect upon this idea, I remember a fact which appears to me to support it strongly. FAHRENHEIT was the first person who discovered that water, when preserved in tranquillity, may be cooled some degrees below the freezing point without freezing. He made the discovery while he was endeavouring to obtain ice from water that had been purged of its air: with this intention he had put some water into little glass globes, and having purged it of air, by boiling and the air-pump, he suddenly sealed up the globes, and then exposed them to the frosty air. He was surprized to find the water remain unfrozen much longer than he expected, when at last he opened some of his globes, in order to apply a thermometer to the water, or otherwise examine what state it was in. The immediate consequence of the admission of the air was a sudden congelation which happened in the water; and in the rest of his globes, a similar production of ice was occasioned by shaking them. The inference that may be drawn from these experiments of FAHRENHEIT's is sufficiently obvious; it appears to me to remove all doubt with regard to the above supposition. Before these experiments of FAHRENHEIT occurred to my memory, I had planned a few, suggested by the above supposition, that might have led to the same conclusion; but the short duration of the frost, for one day only, did not give me time to put them in execution.

XIV.

E X P E R I M E N T S

ON THE

D I P P I N G N E E D L E,

MADE BY DESIRE OF THE

R O Y A L S O C I E T Y.

BY

T H O M A S H U T C H I N S.

REDDE, FEBRUARY 16, 1775-



EXPERIMENTS ON THE DIPPING NEEDLE.

Stromness in the isles of Orkney, lat.  $58^{\circ} 59'$  North,  
long.  $3^{\circ} 30'$  West from London, June 9, 1774.

75	50	}	The index placed East.
76	0		
75	45		
75	45		
75	40	}	
74	55	}	The index placed West.
75	0		
75	20		
75	25		
75	25	}	The poles of the needle changed, the index placed West.
76	10		
76	15		
76	25	}	The index facing the East.
77	0		

In these observations the needle was placed horizontal, and the vibration continued between nine and ten minutes. The instrument was set in the middle of a room up one pair of stairs; but being apprehensive that the iron-grate, fender, poker, and tongs, might, in some measure, affect the needle, I determined to make a trial in the open air, and in a place free from such obstacles.

## EXPERIMENTS ON THE DIPPING NEEDLE.

On the Holms in the entrance of Stromness Harbour,  
 June 23, 1774. Variation *per* azimuth  $24^{\circ}$  Westerly.  
 Long. from London  $3^{\circ} 30'$  West, lat.  $58^{\circ} 59'$  North.

76	0	}	The index placed West.
75	40		
75	45		
76	55	}	The index placed East.
76	10		
76	30		
74	45	}	The poles changed, and index East.
75	30		
75	0		
76	0	}	The index placed West.
74	45		
75	0		

The needle in all these observations was left to vibrate from an horizontal position. The instrument was set on the top of the case (in which it was packed) and stood in the open air, in a fine sunny day.

## EXPERIMENTS ON THE DIPPING NEEDLE.

In Hudson's Straits, July 23, 1774, lat.  $62^{\circ} 3'$  North,  
long.  $69^{\circ}$  West from London, variation  $43^{\circ}$  Westerly.

82	50	}	The Index placed East.
82	30		
82	40		
81	50	}	The index placed West.
82	45		
83	45		
82	40		

The needle vibrated from an horizontal situation. These observations were made on a large piece of ice, to which the three ships were grappled. I imagine the first four experiments may be depended on, as it was calm weather; but afterwards, a breeze springing up gave the ice a circular motion, which made it impossible to keep the instrument exactly in the magnetic meridian, as may be seen in the fifth and sixth experiments. I re-adjusted the instrument for the last observation; but finding the ice still continued in motion, I judged it unnecessary to make any farther experiments at this time, as it could not be done with that exactness I could wish, or give satisfaction to the learned Society to whom I have the honour of transmitting these remarks.

## EXPERIMENTS ON THE DIPPING NEEDLE.

In Hudson's Straits, July 27, 1774, lat.  $62^{\circ} 23'$  North,  
 long.  $71^{\circ} 30'$  West from London, variation  $42^{\circ} 50'$   
 Westerly *per* azimuth.

81	45	}	The index placed East.
83	12		
82	12		
83	0	}	The index placed West.
82	45		
83	45		
83	30	}	The poles changed, and index West.
84	0		
83	35		
85	0	}	The index placed East.
83	25		
83	45		

I met with the same impediment as in the last trial of the instrument, the field of ice turning round, so as to remove the needle constantly out of the magnetic meridian; however, I endeavoured to be exact, and re-adjusted the position of the instrument twice during the observations.

## EXPERIMENTS ON THE DIPPING NEEDLE.

In Hudſon's Straits, July 28, 1774, lat.  $62^{\circ} 25'$  North,  
long.  $71^{\circ} 30'$  Weſt from London, variation *per* azi-  
muth  $44^{\circ}$  Weſt.

83    0    } The index pointing Weſt.  
83    0    }

83 | 30    } The index pointing Eaſt.  
83    0    }

81    30    } The poles changed, and index Eaſt.  
81    40    }

82    8    } The index pointing Weſt.  
82    0    }

Theſe obſervations were made with the aſſiſtance of Captain Richards, on a table in the cabin of the Prince Rupert. We uſed all imaginary care to render them exact; yet, at the concluſion, I found the ſhip, though faſt to a field of ice, had altered the poſition of her head: for by placing the index to the North line of the inſtrument, the needle ſtood at  $88^{\circ} 10'$  Weſt, inſtead of being perpendicular at  $90^{\circ}$ .

## EXPERIMENTS ON THE DIPPING NEEDLE.

In Hudfon's Bay, August 14, 1774, lat.  $56^{\circ} 53'$  North,  
 long.  $85^{\circ} 22'$  West from London, variation *per* azi-  
 muth  $24^{\circ}$  West.

o	'	
82	o	} The index placed East.
82	o	
82	15	} The index placed West.
82	20	
82	50	} The poles changed, and index East
82	35	
82	30	} The index placed East.
82	15	

These experiments were made in conjunction with Captain Richards, in the cabin of the Prince Rupert, whilst she lay amongst ice. The ship frequently varied the position of her head a point of the compass; but by replacing the instrument as often as we found occasion, I have the greatest reason to think these observations (which took up above three hours) are pretty accurate.

EXPERIMENTS ON THE DIPPING NEEDLE.

At Moofe Fort in Hudson's Bay, September 8, 1774, lat.  $51^{\circ} 20'$  North, long.  $82^{\circ} 30'$  West from London, variation  $17^{\circ}$  West.

80° 25'	}	The index placed West.
80 15		
80 35		
79 0		
80 30	}	The Index placed East.
81 25		
80 13		
81 13		
79 10	}	The poles reversed, index East.
80 45		
79 50		
79 10		
79 10	}	The index placed West.
80 25		
79 45		
80 5		

The observations were made on shore. So remarkable difference between them, when I was expecting quite the reverse, surprized me as much as the increased inclination of the needle from observations made nearly in the same parallel of latitude in London. I endeavoured, by drawing a magnetical meridional line with chalk, and paying the greatest attention to keeping the instrument perfectly steady and horizontal, to render these experiments accurate, and fulfil the intention of the Royal Society.

## EXPERIMENTS ON THE DIPPING NEEDLE.

At Albany Fort in Hudson's Bay, September 14, 1774;  
 long.  $82^{\circ} 30'$  West, lat.  $52^{\circ} 22'$  North, variation  $17^{\circ}$   
 West.

o 1

80 13

80 25

79 37

79 55

I made a trial of the instrument at this place, but having lost the slip of paper on which I had noted the experiments, I was dubious whether I should insert the above or not. I can only recollect these four, and am not positive which way the index stood; however, I remember that the mean of all the observations I made was something less than  $80^{\circ}$ . Time will not permit me to repeat the operation during the ship's stay in these parts; I must therefore defer it to a future period. During the winter, I shall have frequent opportunities of amusing myself this way; and the respect I bear the Royal Society, makes every service I render to that illustrious body an additional happiness to me.

Their devoted servant,

THOMAS HUTCHINS,

Albany Fort,  
 September 17, 1774.



## OBSERVATIONS ON HOY, 1774.

Month.	Hour.	Barometer.	Thermometer.	Weather.	Circumstances.
1774. June 11.	0 15	28,63	59	Clear.	On the top of the hill.
	0 30	28,60	56½	Foggy.	Ditto.
	4 15	30,22	63	Clear.	At low water mark.

Hoy is a remarkable high hill near Stromness, in the Orkneys, and is placed by Mr. MACKENZIE in lat. 58° 58' North, and long. 3° 30' West from London. The two first observations were made on the highest part of the hill. Soon after the first, a fog was seen below arising from the water, at length it reached the summit of the hill; the air seemed very raw and cold to the touch, and the instruments shewed as in the second observation. The barometer continued at 28,60 inches after the fog was gone off, but the thermometer rose two or three degrees. The last observation was made at low water mark, about half a mile from the bottom of the hill. THOMAS HUTCHINS.

*“The height of Hoy above low-water mark, according to these observations should be 249,93 fathoms, or as near as may be 500 yards, neglecting the correction for the difference that may be supposed in the temperature of the quicksilver at the two stations, the quantity of which is uncertain.”* S. HORSLEY.

A  
METEOROLOGICAL JOURNAL

FOR THE YEAR 1774.

KEPT

AT THE ROYAL SOCIETY'S HOUSE

BY ORDER OF

THE PRESIDENT AND COUNCIL.

# METEOROLOGICAL JOURNAL

## for 1774.

		Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
				without	within.					
		H. M.				Inches.	Inch.	Points.	Str.	
Jan.	1	8	0	26,0	36,0	29,57		W	1	Frost.
		2	30	31,5	36,5	29,55				
	2	8	30	30,8	34,5	29,44		NW	1	Fair.
	3	8	15	26,0	32,5	29,765		NNW	1	Fair.
		2	10	32,5	33,0	29,89				
	4	8	40	24,0	31,0	30,015		SW	1	Fine.
		2	0	32,0	32,0	29,995		WSW	1	
	5	8	15	25,8	32,0	30,135		SW	1	Fog.
		4	0	30,0	32,0	30,175		SW	1	Fog.
	6	8	5	34,7	33,0	29,975		SW	1	Rain.
		2	10	41,0	35,5	29,925		SSW	1	Rain.
	7	8	5	35,7	36,5	29,585	0,596	NNW	2	Clear.
		2	0	39,0	38,5	29,67		NW	1	
	8	8	15	28,0	36,0	29,93		SSW	1	Fog.
		3	15	36,0	37,0	29,84		E		
	9	8	15	37,5	38,0	29,475	0,956	N	1	Rain.
		2	0	33,0	38,5	29,615		N	1	Snow.
	10	8	0	30,5	36,5	29,815	0,340	E	1	Clear.
		2	15	31,0	36,5	29,835				
	11	8	0	27,0	33,5	29,735		ENE	1	Fine.
		3	0	32,0	34,0	29,70		ENE		
	12	8	15	28,0	32,0	29,67		ENE	1	Fair.
		2	0	32,5	33,5	29,64		ENE	1	
	13	8	0	30,0	32,0	29,44		E by N	1	Fog.
		2	0	34,0	34,0	29,325				
	14	8	10	42,0	38,0	28,79	0,184	SW	2	Rain.
		2	10	46,0	40,5	28,845		SW		Rain.
	15	8	0	41,5	41,5	29,20	0,042	SW	1	Cloudy.
		2	0	44,7	43,0	29,33		NW	1	
	16	8	15	45,5	44,0	29,265	0,098	SW	1	Cloudy.
		2	10	50,0	45,0	29,22		SW		Cloudy.

## METEOROLOGICAL JOURNAL

for 1774.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
Jan. 17	8	0	48,5	46,0	29,27	0,010	SW	2	Cloudy, windy night.
	2	0	50,5	47,5	28,935		SSW	2	Rain.
18	8	0	34,5	45,0	29,11	0,210	NNE	2	Snow, windy night.
	2	0	35,0	50,5	29,34		NE		
19	8	0	27,0	39,0	29,895		W by N	2	Fair, windy night.
	2	10	34,0	39,0	29,92		NW	1	Fair.
20	8	0	29,5	35,5	29,75		E by S	1	Fog.
	2	0	33,0	36,0	29,65		E by S	1	Fair.
21	8	0	30,0	34,5	29,73		E	1	Fair.
	2	5	35,0	35,5	29,83		E	1	Fair.
22	8	0	27,5	33,5	29,76		E by S	1	Fair.
	2	0	36,0	42,0	29,56		SE	1	
23	8	0	40,5	38,5	28,99	0,136	SW	2	Cloudy.
	3	0	41,0	39,0	29,02		SW	1	Fair.
24	8	0	35,0	38,0	28,85		SW	1	Snow.
	3	0	39,0	39,0	28,98		SW	1	Cloudy.
25	8	0	30,5	37,0	29,455		WSW	1	Fair.
	2	0	36,0	38,0	29,53		WSW	1	Fair.
26	8	0	32,5	37,0	29,35	0,058	SW	1	Fair.
	2	30	39,5	39,0	29,46		WSW	1	Fair.
27	8	0	42,0	39,0	29,50	0,117	SW	1	Rain.
	3	0	48,0	41,5	29,34		SW	1	Rain.
28	8	0	36,0	42,0	29,51	0,131	SW	1	Fair.
	2	10	41,0	43,5	29,56		SW	1	Fair.
29	8	0	37,5	41,5	29,58	0,058	SE	1	Rain.
	3	0	42,5	42,0	29,57		W	1	Fog.
30	8	0	31,5	40,0	29,88	0,022	W	1	Fair.
	2	15	41,0	41,0	29,94		NW	1	Fine.
31	8	0	27,0	37,0	30,055		N by E	1	Fair.
	2	0	35,0	38,0	30,07		N	1	Fair.

## METEOROLOGICAL JOURNAL

for 1774.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
						Points.	Str.	
	H. M.			Inches.	Inch.			
Feb. 1	8 0	31,0	36,0	30,20		NE	1	Fair.
	2 0	32,0	36,0	29,87		SW	1	Snow.
2	8 0	24,5	33,0	30,225		NE	1	Fair.
	2 0	33,5	35,0	30,28		NE	1	Fair.
3	8 0	29,0	33,0	30,355		NNE	1	Fair.
	2 0	37,0	36,0	30,36		NE	1	Thaw.
4	8 30	35,5	36,5	30,435	0,114	NE	1	Fog.
	2 10	40,0	38,0	30,46		NNE	1	Thaw.
5	8 0	29,0	36,0	30,44		E	1	Fair.
	2 0	40,0	38,0	30,44		N	1	Fine.
6	8 0	27,5	35,0	30,31		W	1	Fair.
	2 10	36,5	36,5	30,27		WSW	1	Fine.
7	8 0	36,0	36,0	30,04		WSW	1	Fog.
	2 0	46,0	39,0	29,95		SW by W	2	Fair.
8	8 0	41,0	40,0	29,25	0,211	W	2	Fair.
	2 0	34,0	39,0	29,55		NW	2	Fine.
9	8 0	25,0	35,0	29,92		W	1	Fair.
	3 15	34,0	36,0	29,91		NW	1	Fine.
10	8 0	32,0	35,0	30,00		WSW	1	Fog.
	2 0	38,5	36,5	29,97		S	1	Rain.
11	8 0	45,0	40,0	29,41	0,330	SW	1	Rain, windy night.
	2 0	49,0	42,0	29,30		SW	1	Fine.
12	8 0	40,0	41,5	29,58	0,102	SW	1	Fair.
	3 15	48,5	44,0	29,74		W	1	Fair.
13	8 0	43,5	45,5	29,81	0,040	SW	2	Fair, windy night.
	2 0	51,0	47,0	29,87		SW	1	Fair.
14	8 0	47,0	48,0	29,84		SSW	1	Fair, windy night.
	2 10	52,0	50,0	29,74		SSW	1	Fair.
15	8 0	45,0	48,0	29,74	0,185	SSW	1	Fair.
	2 0	50,0	54,0	29,56		SSW	2	Fair.
16	8 0	48,5	49,0	29,19	0,079	SSW	2	Rain, windy night.
	2 5	47,5	50,0	29,20		SW	2	Rain.

## METEOROLOGICAL JOURNAL

for 1774.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
Feb. 17	8	0	40,0	47,0	29,43	0,057	SW	2	Fair.
	2	0	44,5	47,5	29,55		WSW	2	Fine.
18	8	0	36,0	44,5	29,88		SW	1	Fair.
	2	40	48,5	46,5	29,93		W	1	Fair.
19	8	0	40,5	44,5	30,09		SSW	1	Fair.
	2	0	47,5	46,0	29,96		S	1	Fair.
20	8	0	38,5	45,0	29,97	0,200	SW	1	Fair.
	2	0	48,0	47,0	30,04		SW	1	Fine.
21	8	0	43,0	45,5	29,66		S	2	Fair, windy night.
	2	15	45,0	47,0	29,45		WSW	2	Rain.
22	8	0	39,0	44,0	29,90	0,195	SSW	1	Rain.
	2	0	46,0	45,0	29,61		SSW	2	Rain.
23	8	0	45,0	46,0	29,47	0,127	SW	1	Fair, windy night.
	2	40	50,5	48,0	29,39		SSW	1	Rain.
24	8	0	40,5	47,5	29,16	0,170	SW	3	Fair, windy night.
	2	0	49,0	49,0	29,30		WSW	3	Fair.
25	8	0	39,5	46,5	29,26	0,090	E	1	Rain, windy night.
	2	0	40,5	46,0	28,95		N	1	Rain.
26	8	0	34,5	42,0	29,55	0,280	W by N	1	Fair.
	2	0	41,0	43,0	29,68		W by N	1	Fine.
27	8	0	33,0	40,0	30,07		WSW	1	Fair.
	2	0	44,5	41,5	30,15		W	1	Fine.
28	8	0	44,5	44,0	29,80	0,180	SW	2	Rain, windy night.
	2	0	52,0	45,5	29,68		SW	2	Fair.

# METEOROLOGICAL JOURNAL

## for 1774.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
						Points.	Str.	
	H. M.			Inches.	Inch.			
Mar. 1	8 0	37,0	43,5	29,85	0,043	WSW	1	Fair.
	2 0	46,0	46,0	29,93		W	2	Fair.
2	8 0	37,5	42,5	29,57		SW	1	Fair.
	2 0	40,0	43,5	29,46		WSW	1	Fair.
3	8 0	33,5	40,5	29,56	0,083	W	1	Fair.
	2 0	43,5	43,0	29,615		NW	1	Fair.
4	8 0	37,5	43,5	29,33	0,020	SE	1	Rain.
	2 20	49,0	46,0	29,16		SW	1	Fair.
5	8 0	41,0	44,0	29,155	0,127	SW	1	Fair.
	2 0	45,5	45,0	29,14		SW	1	Rain.
6	8 0	44,0	45,0	29,34	0,410	SW	1	Fair.
	2 40	54,5	48,0	29,44		SW	1	Fair.
7	7 30	50,0	48,5	29,54	0,142	SW	2	Rain, windy night.
	2 0	51,0	51,0	29,61		SW	2	Rain.
8	7 30	52,0	51,5	29,60	0,328	SW	2	Rain.
	2 0	47,5	52,0	29,73		N	2	Rain.
9	7 30	39,5	47,5	29,94	0,220	NE	2	Rain.
	2 40	40,0	46,0	29,81		NE	2	Rain.
10	7 30	35,0	41,5	29,90	0,387	NE	2	Fair.
	2 0	39,0	48,0	29,89		NE	2	Fine.
11	7 30	34,0	41,5	29,87		NE	2	Fair.
	2 15	42,0	43,5	29,81		NE	2	Fair.
12	8 0	30,0	39,5	29,74		NE	1	Fair.
	2 0	44,0	42,0	29,74		NE	2	Fine.
13	8 0	36,0	40,0	29,97		ENE	1	Fair.
	2 0	41,0	41,5	30,01		E	1	Fine.
14	7 35	31,5	39,0	30,06		NE	1	Fair.
15	7 30	31,5	38,0	29,93		NE	1	Fair.
	2 0	47,0	46,0	29,88		ESE	1	Fine.
16	7 35	32,0	40,0	29,765		ENE	1	Fog.
	2 0	50,5	50,0	29,69		ENE	1	Fine.

## METEOROLOGICAL JOURNAL

for 1774.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Mar. 17	7	30	37,5	43,0	29,62		ENE	1	Fair.
	2	0	60,0	47,0	29,61		SSW	1	Fine.
18	7	30	48,0	49,0	29,55		S	2	Fair.
	2	0	56,5	52,0	29,52		SSE	1	Fine.
19	7	30	45,5	49,5	29,49	0,010	SE	1	Fair.
	2	0	57,5	54,0	29,55		SE	1	Fine.
20	7	35	45,0	49,5	29,73		NE	1	Fair.
	2	0	55,5	53,5	29,78		E	1	Fine.
21	7	35	41,5	49,5	30,01		NE	1	Fair.
	2	0	54,0	52,0	30,09		N	1	Fine.
22	7	30	40,5	49,0	30,30		NE	1	Fair.
	2	0	50,0	51,0	30,31		NNE	1	Fine.
23	7	40	42,0	48,0	30,30		N	1	Fair.
	2	0	46,5	49,0	30,30		NNE	1	Fair.
24	7	30	41,5	47,5	30,33	0,010	E by N	1	Fair.
	2	15	54,0	57,0	30,33		NE	1	Fine.
25	7	0	37,5	47,5	30,22		NNE	1	Fog.
	2	30	53,0	50,0	30,12		NE	1	Fine.
26	7	25	37,5	47,0	30,065		S	1	Fair.
	2	0	56,5	50,0	30,05		WSW	1	Fine.
27	7	40	41,0	48,0	30,07		SSW	1	Fair.
	2	0	59,0	52,0	30,01		S	1	Fine.
28	7	20	46,0	50,0	29,94		NE	1	Fine.
	2	15	60,0	61,5	29,99		NE	1	Fine.
29	7	25	43,0	44,0	30,10		NE by N	1	Fine.
	2	0	57,0	52,5	30,09		NE by N	1	Fine.
30	7	30	41,5	52,0	30,05		N	1	Fine.
	1	45	55,0	56,0	29,98		SW by S	1	Fine.
31	7	30	38,0	49,0	29,90		NE	0	Fog.
	2	0	59,0	56,0	29,725		SE	0	Fine.



# METEOROLOGICAL JOURNAL

for 1774.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
						Points.	S.r.	
	H. M.			Inches.	Inch.			
Apr.	1	7 20	44,0	50,0	29,65	E	1	Fine.
		2 0	54,0	52,0	29,58	E	2	Fine.
	2	7 0	42,0	48,5	29,41	E	1	Fine.
		2 0	53,0	51,0	29,48	E	1	Fine.
	3	7 15	46,5	48,5	29,35	S	1	Fine.
		2 0	54,0	52,0	29,33	SSE	2	Fair.
	4	7 0	43,0	47,0	29,50	0,415 SW	2	Fine.
		2 0	53,0	52,0	29,68	SW	1	Fine.
	5	7 0	42,5	48,0	29,78	SSW	1	Rain.
		2 0	52,5	50,5	29,765	SSW	2	Fine.
	6	7 0	43,5	48,0	29,575	0,122 S	2	Rain.
		2 0	50,0	50,0	29,37	S	2	Rain.
	7	7 0	44,0	48,5	29,35	0,188 S by W	1	Fair.
		2 0	53,0	51,0	29,36	S	2	Rain.
	8	7 0	43,0	48,5	29,39	0,130 S	1	Fair.
		2 15	51,5	51,0	29,45	SSW	1	Fair.
	9	7 0	36,5	47,0	29,56	NE	1	Fog.
		2 0	52,0	49,5	29,56	N	1	Fair.
	10	7 10	44,0	48,5	29,625	W	1	Fair.
		2 0	60,0	51,5	29,69	SSW	1	Fine.
	11	7 0	48,0	50,5	29,805	S	1	Fair.
		2 0	57,5	53,5	29,90	SE	1	Fair.
	12	7 0	51,0	53,5	30,05	SSE	1	Fine.
		2 0	65,0	58,0	30,105	SE	1	Fine.
	13	7 0	46,0	53,0	30,185	N	1	Fine.
		2 0	62,0	58,0	30,21	ENE	2	Fine.
	14	7 0	45,5	51,5	30,24	NE	2	Fair.
		2 0	51,0	53,5	30,21	NE	1	Fine.
	15	7 0	45,0	50,0	29,98	NE	1	Fair.
		2 0	48,5	51,0	29,88	NE	2	Fair.
	16	7 0	42,5	49,0	29,82	NE	1	Fair.
		2 0	48,5	50,0	29,79	N by E	1	Fair.

## METEOROLOGICAL JOURNAL

for 1774.

	Time		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points	Str.	
Apr. 17	7	25	42,0	48,0	29,875		SW	1	Fair.
	2	0	53,0	50,0	29,88		WSW	1	Fair.
18	7	0	47,5	50,0	29,63	0,082	SW	1	Fair.
	2	15	51,5	52,5	29,58		WNW	1	Fair.
19	7	0	38,0	46,0	29,80	0,065	NW	2	Fine.
	2	0	49,0	48,0	29,785		W	2	Fair.
20	7	0	42,0	46,5	29,74		NW	1	Fair.
	2	0	50,0	48,5	29,81		NNE	1	Fine.
21	7	0	38,0	45,0	30,08		W	1	Fine.
	3	0	54,0	49,0	30,08		W	1	Fine.
22	7	0	50,0	50,0	30,04		WSW	1	Fine.
	2	0	57,0	52,5	30,06		WSW	1	Fair.
23	7	0	51,5	53,0	30,11		WSW	1	Fine.
	2	0	67,0	58,0	30,17		WSW	1	Fine.
24	7	30	54,0	55,5	30,175		W	1	Fair.
	2	0	65,5	59,0	30,17		W by S	2	Fair.
25	7	0	51,0	57,5	30,08	0,005	SW	1	Rain.
	2	0	65,0	60,0	29,875		SW	1	Fine.
26	7	0	46,0	56,0	29,65	0,035	W	2	Fine.
	2	0	55,0	56,0	29,615		W	2	Fine.
27	7	0	39,0	50,0	29,605		W	1	Fine.
	2	0	53,0	52,0	29,62		W	1	Fine.
28	7	0	44,5	49,5	29,71		N	1	Fair.
	2	0	43,5	49,0	29,71		N	2	Rain.
29	7	0	46,5	49,5	29,86	0,200	NW	1	Rain.
	2	15	55,5	51,5	29,90		N by W	1	Fair.
30	7	0	50,	53,0	29,925		N	1	Fair.
	2	0	58,5	55,5	29,98		NE	1	Fair.

## METEOROLOGICAL JOURNAL

for 1774.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
						Points.	Str.	
	H. M.			Inches.	Inch.			
May	1	7 0	46,0	52,5	30,005	NE	2	Fair.
		2 0	58,0	55,0	29,97	NE	2	Fine.
	2	7 0	50,0	53,0	29,58	NE	1	Cloudy.
		2 30	60,0	56,0	29,43	SSW	2	Cloudy.
	3	7 10	51,0	55,0	29,38	NW	1	Cloudy.
		2 0	57,0	56,5	29,46	WSW	1	Rainy.
	4	7 0	49,0	54,0	29,635	NE	1	Fine.
		2 6	63,0	60,0	29,65	ENE	2	Fine.
	5	7 0	45,0	52,0	29,52	NE	1	Rainy.
					0,140			
	6	7 0	48,0	53,5	29,53	NE	1	Fine.
		2 0	54,5	55,0	29,58	E	1	Fair.
	7	7 0	46,0	51,5	29,84	NW	1	Fair.
		2 0	56,0	53,5	29,97	W by N	1	Fine.
	8	7 30	47,5	51,5	30,13	ENE	1	Fair.
		2 0	63,0	55,0	30,11	SSE	1	Fine.
	9	7 0	52,0	54,5	30,105	E	1	Fair.
		2 0	66,5	58,0	30,09	E	1	Fine.
	10	7 0	55,5	56,5	29,96	NE	1	Fair.
		2 0	68,0	60,5	29,91	ENE	1	Fine.
	11	7 0	53,0	58,0	29,85	ENE	2	Rain.
		2 0	60,0	60,5	29,89	NE	1	Fine.
	12	7 0	52,5	57,5	29,97	N	1	Fair.
		2 0	65,5	61,0	30,01	N	1	Fine.
	13	7 0	48,0	57,0	30,12	N	1	Fair.
		2 0	64,0	60,0	30,13	N	1	Fine.
	14	7 0	52,0	57,5	30,12	NE	1	Fine.
		2 0	69,0	61,5	30,12	NE	1	Fine.
	15	7 20	53,0	57,5	30,11	NE	1	Fine.
		2 0	66,5	60,5	30,11	NE	1	Fine.
	16	7 0	46,5	56,0	30,175	NE	2	Fine.
		2 15	56,5	57,0	30,175	NE	1	Fine.

## METEOROLOGICAL JOURNAL

for 1774.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds,		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
May 17	7	0	48,0	54,0	30,12		NE	1	Fair.
	2	0	52,5	55,0	30,15		NE	1	Fine.
18	7	0	45,0	51,5	30,15		SSE	1	Fine.
	2	0	50,5	51,5	30,15		NE	1	Fair.
19	7	0	47,0	51,0	30,155		SW	1	Fair.
	2	0	61,0	55,0	30,15		N by W	1	Fine.
20	7	0	52,0	54,5	30,15		E	1	Fine.
	2	0	63,0	57,5	30,09		E	1	Fine.
21	7	0	54,0	55,5	29,75		E	1	Fair.
	2	0	59,5	57,5	29,61		E	1	Fair.
22	7	10	58,5	59,0	29,35		SSW	1	Fine.
	2	0	65,0	61,0	29,34		SSE	1	Fair.
23	7	0	53,0	58,5	29,34		S	1	Fair.
	2	0	54,5	59,0	29,34		S by E	1	Rain.
24	7	0	53,5	57,0	29,52	0,156	S by E	1	Rain.
	2	0	64,0	59,5	29,55		SSW	1	Fair.
25	7	0	50,5	57,0	29,74	0,121	NW	1	Fair.
	2	0	58,0	59,0	29,73		NW	1	Cloudy.
26	7	0	47,0	55,0	29,925	0,093	N	2	Cloudy.
	2	0	53,0	55,5	29,99		N	2	Fair.
27	7	0	45,0	51,0	29,96	0,076	NNW	2	Cloudy.
	2	0	56,0	53,5	29,99		NNW	2	Fair.
28	7	0	47,0	51,5	30,05	0,064	NW	1	Fine.
	1	45	55,0	54,0	30,05		NNW	1	Fair.
29	7	15	48,0	52,5	30,02	0,059	NW	1	Cloudy.
	2	0	50,5	53,5	29,92		NW	1	Rain.
30	7	0	47,0	51,5	29,79	0,050	SW	1	Cloudy.
	2	0	61,5	55,0	29,80		SW	1	Fine.
31	7	0	51,5	54,0	29,81	0,050	S by E	1	Rain.
	2	0	66,5	61,0	29,81		SW	1	Fine.

# METEOROLOGICAL JOURNAL

## for 1774.

		Time.		Therm.		Barom.	Rain.	Winds.		Weather.
		H.	M.	without	within.			Inches.	Inch.	
June	1	7	0	55,5	59,0	29,93	0,050	SSW	1	Fine.
		2	0	69,5	63,0	29,95		SSE	2	Fine.
	2	7	0	63,0	62,0	29,93	0,046	S	2	Fair.
		2	0	66,0	63,5	29,92		S	2	Fine.
	3	7	0	56,0	61,5	29,97	0,854	SW	1	Fine.
		2	15	66,0	63,5	29,88		SW	1	Fair.
	4	7	0	52,0	60,0	29,65		WNW	1	Rain.
		4	30	54,5	51,0	29,65		NNE	1	Rain.
	5	7	0	57,0	60,0	29,69	0,204	WNW	1	Fine.
		2	0	62,5	62,0	29,74		NW	1	Fine.
	6	7	0	56,0	60,5	29,68	0,130	S	1	Fair.
		2	0	67,0	63,5	29,68		E	1	Fine.
	7	7	0	54,5	61,0	30,10		NNW	1	Fine.
		2	0	72,0	64,0	30,17		NNW	1	Fine.
	8	7	10	57,0	62,0	30,34		SE	0	Cloudy.
		2	0	68,5	64,5	30,34		NW	1	Fine.
	9	7	0	65,0	65,0	30,255		W	1	Fair.
		2	0	75,5	68,5	30,24		W	1	Fine.
	10	7	0	61,0	66,0	30,21		NE	0	Fine.
		2	0	64,0	67,0	30,18		E	0	Fair.
	11	7	0	61,5	64,0	30,04		SW	0	Fair.
		2	0	71,0	67,5	29,95		E	1	Fair.
	12	7	0	56,0	62,0	29,75		E	1	Fair.
		2	0	65,0	65,0	29,66		E	1	Cloudy.
	13	7	0	59,5	64,5	29,60		N	2	Fine.
		2	0	70,0	66,0	29,74		WNW	2	Fine.
	14	7	0	58,0	64,0	29,96		SW	1	Fair.
		2	0	72,0	67,0	29,97		SW	2	Fine.
	15	7	0	66,5	67,0	29,85		S	1	Fair.
		2	0	77,0	70,0	29,84		SW	1	Fine.
	16	7	0	62,0	68,0	29,96		S	1	Fine.
		2	0	74,5	69,5	29,99		SW	1	Fine.

## METEOROLOGICAL JOURNAL

for 1774.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
June 17	7	0	63,0	66,0	29,99	0,060	SE	1	Rain.
	2	0	74,5	71,0	29,93		W	1	Fair.
18	7	0	57,0	65,0	30,01	0,170	SW	1	Fair.
	2	0	70,5	68,5	30,07		W	1	Fine.
19	7	30	60,5	66,0	29,90		SE	1	Fine.
	2	0	74,5	69,0	29,84		S	1	Fine.
20	7	0	60,0	65,5	29,475	0,019	S by W	1	Rain.
	2	0	65,0	67,0	29,47		S by W	2	Cloudy.
21	7	0	56,0	63,5	29,54	0,050	SW	1	Cloudy.
	2	10	61,5	64,0	29,64	0,510	S	1	Rain.
22	7	0	54,0	60,0	29,85		NNE	1	Cloudy.
	2	0	59,5	62,0	29,90		NW	1	Cloudy.
23	7	0	54,0	61,0	29,94	0,037	W	1	Cloudy.
	2	0	61,0	61,5	29,98		SW	1	Cloudy.
24	7	0	60,0	61,5	29,92		WSW	1	Fair.
	2	0	69,0	64,0	29,97		SW	1	Fair.
25	7	0	61,0	63,5	30,03		SSW	1	Fair.
	2	0	72,0	67,0	30,00		SSW	1	Fair.
26	7	0	67,0	66,5	29,92		SW	1	Fine.
	2	0	77,5	70,0	29,92		SW	1	Fine.
27	7	0	62,5	68,5	29,90		SW	1	Fine.
	2	0	69,0	69,5	29,87		SW	1	Fair.
28	7	0	60,5	66,5	29,78	0,026	SSW	1	Cloudy.
	2	30	69	67,5	29,80		SW	2	Fair.
29	7	0	59,5	65,5	29,93	0,028	WSW	2	Fine.
	2	10	67,0	65,0	29,94		SW	2	Fine.
30	7	0	58,5	62,5	29,83	0,089	SW	2	Cloudy.
	2	10	67,0	65,0	29,88		SSW	2	Fair.

## METEOROLOGICAL JOURNAL

for 1774.

		Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
				without	within.					
		H. M.				Inches.	Inch.	Points.	Str.	
July	1	7	0	59,0	63,0	30,00		SW	1	Cloudy.
		2	0	73,5	66,5	30,01		SW	1	Fine.
	2	7	0	62,0	65,5	30,10		S	1	Fair.
		2	0	76,0	69,0	29,92		SW	1	Fine.
	3	7	0	61,0	67,5	29,71		S	1	Cloudy.
		2	0	75,5	70,5	29,74		SW	2	Fine.
	4	7	0	60,0	67,0	29,84		SW	1	Fine.
		2	0	68,5	68,5	29,88		SW	2	Fine.
	5	7	0	59,0	65,0	29,98	0,045	SSW	1	Fair.
		2	0	68,0	67,0	29,92		SSW	2	Fair.
	6	7	0	58,0	65,0	29,77	0,178	WSW	1	Fine.
		2	0	63,0	65,0	29,75		WSW	2	Fine.
	7	7	0	56,0	62,0	30,04	0,208	W	1	Fair.
		2	0	72,5	66,5	30,05		WSW	1	Fine.
	8	7	0	64,0	63,0	29,93		SSE	1	Fine.
		2	30	68,5	66,0	29,92		SSE	1	Fair.
	9	7	0	57,5	64,5	29,98	0,060	SW	1	Fine.
		2	0	64,5	66,5	29,99		SW	2	Rain.
	10	7	20	59,0	63,0	29,99	0,226	SSW	1	Fine.
		2	5	69,0	65,0	29,92		SSW	1	Fine.
	11	7	0	59,5	64,0	29,96	0,062	SW	1	Fair.
		2	0	60,5	64,0	29,99		NW	1	Rain.
	12	7	0	58,0	63,0	30,03	0,130	SW	1	Fair.
		2	0	67,5	65,0	29,98		SW	2	Fine.
	13	7	0	61,0	64,0	29,77	0,053	WSW	1	Fair.
		2	0	68,5	66,0	29,90		WNW	1	Fair.
	14	7	0	61,0	64,0	29,61	0,282	SSW	1	Cloudy.
		2	10	63,0	65,0	29,80		NW	2	Fine.
	15	7	0	58,5	63,0	30,08	0,039	NW	2	Fine.
		2	0	69,0	65,0	30,19		NW	2	Fine.
	16	7	0	56,5	62,0	30,32		NW	1	Fine.
		2	5	71,0	65,5	30,30		NW	1	Fine.

## METEOROLOGICAL JOURNAL

for 1774.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
July 17	7	20	60,0	65,0	30,235		WSW	1	Fine.
	2	20	68,5	67,0	30,21		SW	1	Fair.
18	7	0	60,0	65,0	30,19		WNW	1	Fair.
	2	0	68,0	67,0	30,21		WNW	1	Fair.
19	7	0	59,5	63,5	30,16		SW	1	Fair.
	2	0	66,0	65,5	30,05		WSW	2	Fair.
20	7	0	58,5	61,5	29,87	0,168	WNW	1	Fine.
	2	0	64,0	64,5	29,92		NW	1	Fine.
21	7	0	55,5	60,0	30,03	0,063	SW	1	Fair.
	2	0	70,5	64,5	30,03		SW	1	Fair.
22	7	0	57,0	62,5	30,26	0,102	N	1	Fair.
	2	0	67,5	65,0	30,33		N by E	1	Fine.
23	7	0	63,0	65,0	30,36		WSW	1	Fair.
	2	0	77,5	68,0	30,33		S by W	1	Fine.
24	7	0	60,0	66,5	30,22		S	0	Fine.
	2	0	77,5	69,0	30,14		S by W	0	Fine.
25	7	0	63,0	68,5	30,07		S	0	Fair.
	2	0	81,0	72,0	30,05		S by E	0	Fine.
26	7	0	63,0	70,0	29,92		NE	1	Fine.
	2	0	83,5	73,5	29,78		E by S	1	Fine.
27	7	0	61,0	70,0	29,92	0,540	S by W	1	Fine.
	2	0	71,0	70,0	29,95		SW	2	Fine.
28	7	0	58,0	65,0	29,88	0,192	WSW	1	Fine.
	2	0	69,5	68,0	29,92		SW	1	Fine.
29	7	10	62,0	65,0	29,83	0,045	WSW	2	Rainy.
	2	0	68,0	68,0	29,79		SW	2	Fair.
30	7	0	60,5	66,5	29,94	0,045	W	1	Fine.
	2	0	72,5	69,0	30,01		NW	1	Fair.
31	7	15	60,0	66,5	30,01		W by S	1	Fair.
	2	0	70,0	68,0	30,03		NW	1	Fine.



# METEOROLOGICAL JOURNAL

## for 1774.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Aug. 1	7	0	56,5	64,5	30,12	0,068	N by W	1	Fair.
	2	0	69,0	67,5	30,20		NNE	1	Fine.
2	7	0	53,5	63,0	30,32		NNE	1	Fine.
	2	0	67,0	64,5	30,31		SE	1	Fine.
3	7	0	55,0	62,5	30,22		S by W	1	Fine.
	2	0	73,0	65,5	30,15		N	1	Fine.
4	7	0	59,0	65,0	29,96	0,030	E by N	2	Fair.
	2	0	68,5	68,0	30,02		NE	1	Fair.
5	7	0	56,5	64,0	29,88	1,002	NE	2	Fair.
	2	0	62,5	65,5	30,00		NW	1	Fair.
6	7	0	59,0	63,5	30,09	0,042	SW	1	Cloudy.
	2	0	73,0	68,0	30,095		WSW	2	Fine.
7	7	0	63,5	66,0	30,09		WSW	2	Fine.
	2	0	78,0	70,5	30,11		WSW	2	Fine.
8	7	0	61,5	67,0	30,11		N	1	Fair.
	2	0	77,0	73,0	30,04		E by S	1	Fair.
9	7	10	67,5	70,5	29,81		S	1	Fair.
	2	0	77,5	74,0	29,80		WSW	1	Fine.
10	7	0	57,0	68,5	29,88		WSW	1	Fine.
	2	0	69,5	68,5	29,91		W	1	Fair.
11	7	0	54,0	63,0	30,08	0,138	SW	1	Fine.
	2	0	69,0	66,0	30,17		WSW	1	Fine.
12	7	0	54,5	62,0	30,22		SW	1	Fine.
	2	0	69,0	66,5	30,23		NW	1	Fine.
13	7	0	59,0	62,0	30,20		SW	1	Fair.
	2	30	71,5	68,5	30,20		W	1	Fine.
14	7	0	59,0	69,5	30,02		SW	1	Fine.
	2	0	73,0	68,5	29,92		SW	1	Fine.
15	7	0	59,0	65,5	29,68	0,106	SSW	1	Fine.
	2	0	68,0	67,5	29,63		SSW	2	Fair.
16	7	0	59,0	64,5	29,81	0,065	SE	1	Rain.
	2	0	78,0	67,0	29,63		S	2	Fair.

## METEOROLOGICAL JOURNAL

for 1774.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Aug. 17	7	0	58,0	64,0	29,68	0,048	W by S	2	Fine.
	2	0	69,5	66,0	29,84		W	2	Fine.
18	7	0	56,5	63,5	29,97		WSW	1	Fine.
	2	0	57,0	64,0	29,99		SW	1	Fine.
19	7	0	55,5	62,5	30,19		N by E	1	Fair.
	2	0	71,5	66,5	30,21		SE	1	Fine.
20	7	0	60,0	63,5	30,14		E by N	1	Fine.
	2	0	71,5	69,0	30,10		ESE	2	Fine.
21	7	0	62,5	66,0	30,04		SE	1	Fine.
	2	0	77,0	68,5	30,03		SE	1	Fine.
22	7	0	61,0	66,5	29,98		ENE	1	Cloudy.
	2	0	69,5	68,5	30,00		NNE	1	Fair.
23	7	0	59,0	66,0	30,11		N	1	Cloudy.
	2	0	68,0	68,0	30,15		N	1	Fair.
24	7	0	56,0	64,5	30,17		NNE	1	Cloudy.
	2	0	65,0	65,0	30,16		N by E	1	Cloudy.
25	7	0	57,5	63,0	29,93		E by N	1	Fair.
	2	0	73,0	66,5	29,795		S	1	Fine.
26	7	0	59,0	65,0	29,55	0,544	W by S	1	Fair.
	2	0	70,5	67,5	29,62		SW	1	Fine.
27	7	0	55,5	64,0	29,56	0,291	W by S	1	Fine.
	2	0	63,0	66,0	29,64		NW	1	Thunder.
28	7	0	57,0	52,5	29,535	0,293	S by E	1	Cloudy.
	2	0	62,5	63,5	29,38		W by S	2	Fair.
29	7	0	52,0	60,5	29,64	0,453	SSW	1	Fine.
	2	30	57,0	61,5	29,72		S by W	1	Fair.
30	7	0	58,0	60,5	29,80	0,165	S by W	1	Rain.
	2	0	67,0	63,0	29,62		SW	2	Cloudy.
31	7	0	63,5	64,0	29,82	0,095	S by W	2	Cloudy.
	2	0	67,0	66,0	29,86		SSW	2	Fair.

# METEOROLOGICAL JOURNAL

## for 1774.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H. M.		without	within.	Inches.	Inch.	Points.	Str.	
Sept. 1	7	0	64,5	66,5	30,01	0,022	SSW	1	Cloudy.
	2	0	67,5	69,5	30,02		SW	2	Fine.
2	7	0	64,0	67,5	29,825		S	1	Fair.
	2	0	68,0	69,0	29,90		W	1	Fair.
3	7	0	53,0	64,5	30,09	0,045	SSW	1	Fair.
	2	0	70,0	67,0	29,96		SSW	1	Fine.
4	7	0	61,5	64,0	29,85		SW	1	Fine.
	2	0	73,0	68,0	29,81		SW	1	Fine.
5	7	0	52,0	61,5	30,00	0,041	W by N	1	Fine.
	2	0	68,0	64,5	30,035		W	1	Fine.
6	7	0	60,5	62,5	29,83	0,068	S by W	1	Cloudy.
	2	0	70,0	67,0	29,90		WNW	1	Fair.
7	7	0	54,0	61,0	30,05	0,346	NW	1	Cloudy.
	2	0	62,0	62,5	30,11		NNW	1	Fair.
8	7	0	45,5	53,5	30,27		NNE	1	Fine.
	2	0	57,0	56,5	30,28		N by E	1	Fine.
9	7	0	42,5	49,5	30,15		NNE	1	Fine.
	2	0	60,0	55,5	30,08		N by E	1	Fine.
10	7	0	50,5	54,5	30,06		E by N	1	Fine.
	3	0	67,0	58,5	30,04		SSW	1	Fair.
11	7	0	52,5	57,5	30,01	0,116	WSW	1	Fine.
	2	0	66,0	61,0	30,01		W by S	1	Fine.
12	7	0	53,0	60,0	29,625	0,176	SSW	1	Fine.
	2	0	59,0	61,5	29,55		W by S	1	Cloudy.
13	7	0	54,0	59,0	29,405		SE	1	Cloudy.
	2	0	56,0	61,0	29,58		E by N	1	Rain.
14	7	0	51,5	57,0	29,77	0,097	NNW	1	Cloudy.
	2	0	61,0	64,0	29,80		NW	1	Cloudy.
15	7	0	55,0	58,5	29,80		N	1	Cloudy.
	2	0	61,5	60,5	29,87		N by E	2	Fine.
16	7	0	50,0	57,0	29,83		W	1	Fair.
	2	0	58,0	58,5	29,82		N	1	Fine.

METEOROLOGICAL JOURNAL

for 1774.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Sept. 17	7	0	51,0	56,5	29,92	0,179	N	1	Fine.
	2	0	61,5	59,5	29,95		N by E	1	Fine.
18	7	0	53,0	57,5	30,03		NNE	1	Fair.
	2	0	61,5	60,0	30,08		NE	1	Fine.
19	7	0	43,5	54,5	29,98		NE	1	Fine.
	2	30	64,0	58,5	29,87		S	1	Fair.
20	7	0	48,5	56,0	29,68	0,309	WNW	1	Fine.
	2	0	59,5	58,5	29,75		W by N	1	Fine.
21	7	0	52,0	56,0	29,68		SSW	1	Fair.
	2	0	58,0	58,0	29,565		SSE	2	Rain.
22	7	0	48,0	55,5	29,48	0,468	SSE	1	Fine.
	2	0	59,5	58,0	29,29		S by W	2	Fair.
23	7	0	54,0	57,0	29,11	0,244	S by W	2	Fine.
	2	0	58,5	59,0	29,28		S	2	Fine.
24	7	0	55,0	58,0	29,40	0,235	SSE	1	Fine.
	2	0	64,0	60,0	29,42		SE	1	Fair. Thunder about 9 P.M.
25	7	30	53,5	58,5	29,50	0,695	W	1	Rain.
	2	0	59,5	60,5	29,53		W	1	Rain.
26	7	0	53,5	59,0	29,56	0,108	SSW	1	Cloudy.
	2	0	58,5	60,0	29,64		WSW	1	Fair.
27	7	0	47,0	57,0	29,87	0,076	NNE	1	Fog.
	2	0	59,5	59,0	29,90		E	1	Fine.
28	7	0	53,0	56,5	29,78	0,018	S by E	1	Rain.
	2	40	56,0	57,5	29,71		SE	1	Rain.
29	7	0	50,0	56,0	29,68	0,236	S	1	Fine.
	2	0	62,0	59,0	29,65		E	1	Fair.
30	7	0	53,5	58,0	29,53	0,264	S	1	Fine.
	2	0	56,0	59,5	29,53		S by W	1	Rain.

# METEOROLOGICAL JOURNAL for 1774.

		Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
				without	within.					
		H. M.				Inches.	Inch.	Points.	Str.	
Oct.	1	7	0	49,0	57,5	29,47	0,347	S	I	Fine.
		2	0	58,5	61,0	29,44		SW	I	Rain.
	2	7	20	53,5	57,5	29,40	0,354	SW	I	Fine.
		2	0	57,0	59,0	29,52		NW	I	Fair.
	3	7	0	42,5	53,5	30,12	0,054	W	I	Fine.
		2	0	56,0	55,5	30,22		NW	I	Fine.
	4	7	0	43,5	52,0	30,38		SW	I	Fine.
		2	0	59,0	55,0	30,41		S by W	I	Fine.
	5	7	0	50,5	54,0	30,43		S	I	Fine.
		2	0	60,5	57,5	30,42		S by W	I	Fine.
	6	7	0	52,0	54,0	30,27		SE	I	Rain.
		2	0	64,0	58,0	30,23		SW	I	Fine.
	7	7	0	53,0	57,5	30,32		WSW	0	Fair.
		2	0	64,5	60,5	30,37		W	0	Fine.
	8	7	0	44,5	56,0	30,57		S	0	Fine.
		2	0	57,5	57,5	30,57		NE	I	Fine.
	9	7	40	43,0	53,0	30,49		NE	I	Fair.
		2	20	57,5	55,0	30,38		S	I	Fine.
	10	7	0	47,0	54,0	30,22		SW	I	Fine.
		2	0	59,0	56,0	30,18		W	I	Fine.
	11	7	0	42,5	52,5	30,27		N by E	0	Fine.
		2	0	57,5	55,0	30,27		WSW	I	Fine.
	12	7	0	48,5	54,0	30,29		WSW	I	Fair.
		2	0	59,0	56,0	30,30		NW	2	Fine.
	13	7	0	50,5	55,0	30,40		W	0	Fair.
		2	0	58,5	57,0	30,43		N by E	I	Fair.
	14	7	0	52,0	54,5	30,46		NE	I	Fair.
		2	0	58,0	57,5	30,46		NE	I	Fine.
	15	7	0	41,5	52,0	30,44		NE	I	Fine.
		2	0	55,0	54,0	30,40		E	I	Fine.
	16	7	20	44,0	51,5	30,395		ENE	I	Fine.
		2	0	57,0	53,5	30,24		SE	I	Fine.

## METEOROLOGICAL JOURNAL

for 1774.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Oct. 17	7	0	42,5	51,5	30,12		ENE	1	Fog.
	2	0	54,0	53,5	30,14		S by E	1	Fine.
18	7	0	47,0	52,5	30,26		S by W	1	Fog.
	2	0	58,5	55,0	30,23		SW	1	Fair.
19	7	0	52,0	54,5	30,27		WSW	1	Fair.
	2	0	59,0	50,5	30,27		SW	1	Fair.
20	7	0	48,5	55,0	30,31		SSW	1	Fine.
	2	0	60,5	59,0	30,28		S	1	Fine.
21	7	0	44,5	53,5	30,24		N	1	Fair.
	2	0	60,5	56,0	30,22		S by E	0	Fine.
22	7	0	50,0	54,5	30,15		S by W	0	Fair.
	2	20	60,5	57,5	30,05		SW	1	Fair.
23	8	0	52,0	56,0	29,95	0,089	NW	1	Rain.
	2	0	50,0	55,0	29,95		SE	1	Rain.
24	7	0	45,0	52,5	29,80	0,246	NW	1	Fair.
	2	20	54,5	53,0	29,80		S by W	1	Fine.
25	7	0	41,0	50,0	29,87		SW	1	Fine.
	2	0	51,5	51,5	29,92		NW	1	Fine.
26	7	0	36,0	47,5	30,10		SW	1	Fine.
	2	15	51,5	49,0	30,15		NW	1	Fine.
27	7	0	38,5	46,5	30,15		N	1	Fine.
	2	0	52,0	48,5	30,12		N by E	1	Fine.
28	7	0	38,5	45,5	30,11		NNE	1	Fine.
	2	0	49,0	48,5	30,08		NE	1	Fair.
29	7	0	39,0	45,5	29,87	0,076	NE	1	Cloudy.
	2	0	45,0	47,0	29,74		E by N	1	Fair.
30	8	0	44,5	45,5	29,54	0,010	E by N	1	Rain.
	2	0	49,5	47,5	29,53		E by N	1	Fair.
31	7	0	48,5	49,0	29,50	0,172	E	2	Cloudy.
	2	20	54,0	51,0	29,54		ESE	1	Rain.

## METEOROLOGICAL JOURNAL.

for 1774.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Nov.	1	7 0	51,5	52,0	29,75	0,082	NE	0	Fog.
		2 0	58,5	54,0	29,86		SSE	1	Fair.
	2	7 30	49,0	53,0	30,00	0,030	E by N	1	Fog.
		2 0	53,0	53,5	30,00		E by N	1	Fair.
	3	7 30	52,5	54,0	29,97		SE	0	Fog.
		2 0	58,0	56,0	29,98		SSW	1	Fair.
	4	7 30	47,5	53,5	30,01		E by N	1	Fog.
		2 20	54,0	54,5	29,96		ENE	1	Fair.
	5	7 30	48,5	52,5	29,71		SE	1	Fair.
		2 0	50,0	52,5	29,56		E	1	Fair.
	6	8 10	47,0	51,0	29,17	0,580	E by N	2	Rain.
		2 15	48,5	51,5	29,20		E	2	Rain.
	7	7 30	41,5	47,5	29,51	0,313	E by N	1	Fair.
		2 0	44,0	48,0	29,57		NE	1	Fair.
	8	7 30	40,0	45,0	29,455		NNE	1	Fair.
		2 0	44,0	46,0	29,44		N	2	Fair.
	9	7 30	39,0	43,5	29,50		NNE	1	Fair.
		2 30	42,0	44,5	29,53		ENE	2	Rain.
	10	7 30	33,0	42,0	29,945	0,034	N	1	Fair.
		2 0	41,5	43,0	30,125		N	1	Fine.
	11	7 30	35,0	40,5	30,05		NE	1	Fair.
		2 0	40,0	41,5	29,92		ENE	1	Little snow.
	12	7 30	32,5	39,0	29,935	0,063	SW	1	Fog.
		2 0	41,0	41,0	29,86		SW	1	Fair.
	13	7 35	33,0	38,0	30,18		N	1	Fine.
		2 0	40,5	40,0	30,225		NNW	1	Fine.
	14	7 30	34,0	37,5	30,18		SW	0	Rain.
		2 0	41,0	41,0	30,19		SW	1	Fair.
	15	7 30	42,5	42,0	30,11		S by W	1	Fair.
		2 0	49,0	44,5	30,07		WSW	1	Rain.
	16	7 40	47,5	40,5	30,06	0,010	SSW	1	Fine.
		2 0	56,0	49,0	30,07		W by S	1	Fair.

## METEOROLOGICAL JOURNAL

for 1774.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Nov. 17	7	3	48,0	50,0	30,07		SW	1	Fair.
	2	0	52,5	51,0	29,97		SW	1	Fine.
18	8	0	51,0	51,5	29,55	0,115	W by S	2	Fair.
	2	15	44,0	50,0	29,66		W by N	2	Fair.
19	7	40	34,5	43,5	29,81		NW	2	Fine.
	2	0	39,5	43,0	29,82		W by N	1	Fine.
20	8	30	33,0	40,0	29,96		N	1	Fine.
	2	0	39,0	41,0	30,00		N	1	Fine.
21	7	30	33,5	38,5	30,11	0,024	N	1	Fair.
	2	0	36,5	39,0	30,07		N	1	Fair.
22	7	30	33,0	37,5	30,00		W by S	1	Snow.
	2	0	34,5	37,5	29,74		SSW	1	Snow and Rain.
23	7	30	35,5	39,0	29,865	0,181	E	1	Fair.
	2	20	38,0	40,5	29,865		W by S	1	Fair.
24	7	30	40,0	41,0	29,465	0,081	N by W	2	Fair, much wind last night.
	2	15	35,0	41,0	29,38		N	2	Rain and Snow.
25	8	0	32,5	37,5	29,635	0,094	NNE	1	Snow, windy night.
	2	0	36,0	39,0	29,76		N	2	Fair.
26	8	0	32,5	37,5	29,94	0,020	N	1	Fair.
	2	0	35,5	38,5	29,98		N	1	Fine.
27	8	0	31,5	36,0	30,14		N	2	Fair.
	2	0	35,0	36,5	30,11		N	1	Fair.
28	8	0	32,5	34,5	29,74		S	1	Snow.
	2	0	41,0	36,5	29,48		S	1	Fair.
29	8	0	33,0	36,5	29,28		SW	1	Fair.
	2	0	39,0	37,5	29,24		SW	1	Fair.
30	8	0	32,5	36,0	29,31		SW	1	Fine.
	2	30	38,5	37,5	29,40		SW	1	Fine.



## METEOROLOGICAL JOURNAL

for 1774.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Dec. 1	8	0	31,0	36,5	29,57		W by S	1	Fair.
	2	0	38,5	37,5	29,62		NW	1	Fine.
2	8	0	34,0	36,5	29,45		SE	1	Fair.
	2	20	40,0	39,5	29,25		SSE	1	Rain.
3	8	0	45,5	40,5	29,11	0,283	SE	1	Rain.
	2	0	48,0	43,0	29,15		SSE	1	Rain.
4	8	0	44,5	45,5	29,48	0,068	E	1	Fair.
	2	0	49,0	47,0	29,68		SE	1	Fair.
5	8	0	43,0	47,0	29,84	0,075	ENE	1	Fair.
	2	0	47,0	47,5	29,90		E	1	Fine.
6	8	0	39,0	43,5	30,13		E	1	Fog and Rain.
	2	0	35,5	41,5	30,18		NE	2	Fine.
7	8	0	31,0	36,5	30,39		NE	1	Fair.
	2	0	32,0	36,5	30,35		NE	1	Fine.
8	8	0	28,5	33,5	29,99		N	1	Fine.
	2	0	29,5	34,0	29,88		N	1	Fine.
9	8	0	25,0	32,5	29,61		NW	1	Fair.
	2	0	27,0	32,5	29,61		NW	1	Snow.
10	8	0	29,0	30,5	29,34		NE	1	Fair.
	2	0	34,0	33,5	29,32		E by N	1	Rain.
11	8	0	45,5	37,5	29,40	0,327	S	2	Rain.
	2	0	51,5	41,0	29,54		S	2	Fair.
12	8	0	50,5	44,5	29,71		SSW	1	Fair.
	2	0	53,5	46,5	29,71		SE	1	Fine.
13	8	0	44,0	47,5	30,08		S	1	Fine.
	2	0	49,5	48,5	30,10		SW	1	Fine.
14	8	0	47,5	48,5	30,02	0,089	W by S	0	Fog.
	2	0	50,0	49,0	29,98		SW	0	Rain.
15	8	0	42,0	48,0	29,935	0,927	SW	1	Fog.
	2	0	48,5	49,0	30,09		W by N	1	Fine.
16	8	0	37,5	45,5	30,22		NE	1	Fog.
	2	0	43,5	46,5	30,16		SE	1	Fair.

## METEOROLOGICAL JOURNAL

for 1774.

	Time.		Therm.	Therm.	Barom.	Rain.	Winds.		Weather.
	H.	M.	without	within.	Inches.	Inch.	Points.	Str.	
Dec. 17	8	0	40,5	44,5	30,08		E	0	Fog.
	2	30	44,0	46,5	30,09		ENE	1	Fog, small rain.
18	8	0	42,5	44,5	30,22		W	0	Fog.
	2	30	47,0	45,5	30,26		SW	1	Fair.
19	8	0	45,0	46,0	30,31		NW	1	Fog.
	2	0	47,5	47,0	30,31		NW	1	Fair.
20	8	0	39,5	45,5	30,30		NE	1	Fog.
	2	0	40,0	45,0	30,30		NE	1	Fair.
21	8	0	37,0	42,5	30,21		NNE	0	Fog.
	2	0	38,5	42,5	30,19		NNE	0	Fog.
22	8	0	34,0	40,5	30,35		NNE	1	Fine.
	2	0	40,5	41,0	30,42		NNE	1	Fine.
23	8	0	37,0	40,5	30,65		N	0	Fog.
	2	0	40,0	41,0	30,70		N	1	Fair.
24	8	0	39,5	33,5	30,70		N	1	Fair.
	2	10	37,0	40,0	30,71		NNE	1	Fine.
25	8	15	27,0	36,0	30,675		W	1	Fog.
	2	0	30,5	36,0	30,65		W by N	1	Fog.
26	8	0	34,5	36,0	30,65	0,010	W	0	Fog.
	2	0	36,5	36,5	30,61		W	0	Fog.
27	8	0	35,5	37,0	30,58		W	0	Fog.
	2	0	36,5	37,5	30,56		W by N	1	Fair.
28	8	0	36,0	37,5	30,47		W	0	Fog.
	2	0	39,0	38,5	30,39		W	1	Fair.
29	8	0	35,0	38,5	30,49		W	1	Fine.
	2	0	40,0	39,5	30,545		W	1	Fine.
30	8	0	28,0	36,5	30,56		NW	1	Fog.
	2	0	34,0	36,5	30,49		NW	1	Fine.
31	8	0	27,0	33,5	30,21		W by N	1	Fine.
	2	0	36,0	35,0	30,04	0,027	W by N	1	Fair.

1774	Thermometer without.					Therm. within.			Barometer.		
	Greatest height.	Least height.	Mean height A. M.	Mean height P. M.	Mean whole day.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.
January	50,5	24,0	30,0	37,7	35,3	50,5	27,0	37,4	30,175	28,79	29,57
February	52,0	24,5	37,6	43,5	40,5	50,0	33,0	42,4	30,46	29,16	29,806
March	60,0	33,5	39,6	50,5	45,0	61,5	38,0	47,4	30,33	29,14	29,82
April	67,0	36,5	44,9	54,8	49,8	60,0	45,0	51,3	30,24	29,33	29,786
May	69,0	45,0	49,7	59,9	54,8	61,5	51,0	55,9	30,175	29,34	29,871
June	77,5	52,0	59,1	68,4	63,7	71,0	59,0	64,6	30,34	29,47	29,90
July	83,5	55,5	59,7	70,1	64,9	73,5	50,0	65,9	30,36	29,61	30,00
August	78,0	73,0	58,2	69,2	63,7	73,0	52,5	65,6	30,32	29,38	29,954
September	73,0	42,5	52,6	62,1	57,3	69,5	49,5	59,7	30,28	29,11	29,795
October	64,5	36,0	46,0	56,3	51,1	61,0	45,5	53,6	30,57	29,40	30,13
November	58,5	41,0	39,2	43,5	41,3	56,0	34,5	43,7	30,225	29,17	29,807
December	53,5	25,0	37,3	40,8	39,7	49,0	30,5	40,8	30,71	29,11	30,09

inch.

The quantity of rain in the whole year was 26,328, or about  $26\frac{1}{3}$  inches.

FOR THE VARIATION OF THE MAGNETIC NEEDLE.

OBSERVATIONS.

		Morning.	Noon.	2 P. M.	Evening.
		° /	° /	° /	° /
August	21	21 27			
	22	21 20		21 37	21 15.
	23	21 28.	21 38	21 24	21 18
	24	20	33	33	18
	25	41	35	35	19.
	26	22	32	32	20
	27	21	35	29	20.
	28	20	32	10	20
	29	15.	33	15	02.
	30	34	34.	15	14.
	31	23		34	12
September.	1	27.	34	34.	12
	2	22	35.	32	19.
	3	41.	36	33.	18
	4	21	16	34	29.
	5	24			
		406	393	397.	236
		21 25	21 33	21 28	21 17.
The mean of all,					21. 26.
Error of instrument,					10—
Correct variation,					21. 16 W.

How the error of the instrument was found, will be shown in the next publication.



PHILOSOPHICAL  
TRANSACTIONS,  
GIVING SOME  
ACCOUNT  
OF THE  
Present Undertakings, Studies, and Labours,  
OF THE  
INGENIOUS,  
IN MANY  
CONSIDERABLE PARTS OF THE WORLD.

---

VOL. LXV. For the Year 1775.

PART II.

---

LONDON,

PRINTED BY W. BOWYER AND J. NICHOLS:  
FOR LOCKYER DAVIS, IN HOLBOURN,  
PRINTER TO THE ROYAL SOCIETY.

---

MDCCLXXV.



# C O N T E N T S

O

## V O L. LXV. P A R T II.

- XVI. *A*N abridged State of the Weather at London in the Year 1774, collected from the Meteorological Journal of the Royal Society. By S. Horsley, LL.D. Sec. R. S. page 167
- XVII. *Extract of a Meteorological Journal for the Year 1774, kept at Bristol, by Samuel Farr, M. D.* p. 194
- XVIII. *Extract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1774. By Thomas Barker, Esquire. Communicated by Sir John Pringle, Bart. P. R. S.* p. 199
- XIX. *An Account of some Thermometrical Observations, made by Sir Robert Barker, F. R. S. at Allahabad in the East Indies, in Lat. 25° 30' N. during the Year 1767, and also during a Voyage from Madras to England, in the Year 1774. Extracted from the original Journal by the Hon. Henry Cavendish, F. R. S.* p. 202
- XX. *A Second Essay on the Natural History of the Sea Anemonies. By the Abbé Dicquemare, Member of several Academies,*



- Academies, and Professor of Natural Philosophy, &c. at Havre de Grace.* p. 207
- XXI. *Account of the Sea-Cow, and the Use made of it.*  
By Molineux Shulldham, Esquire. Communicated by  
the Hon. Daines Barrington, F. R. S. p. 249
- XXII. *The Process of making Ice in the East Indies.* By  
Sir Robert Barker, F. R. S. in a Letter to Dr.  
Brocklesby. p. 252
- XXIII. *Of the House-Swallow, Swift, and Sand-Martin.*  
By the Rev. Gilbert White, in Three Letters to the Hon.  
Daines Barrington, F. R. S. p. 258
- XXIV. *Account of a Machine for raising Water, executed  
at Oulton, in Cheshire, in 1772.* In a Letter from  
Mr. John Whitehurst to Dr. Franklin. p. 277
- XXV. *Extract of a Letter from Mr. Lexel to Dr. Morton.*  
Dated Petersburg, June 14, 1774. p. 280
- XXVI. *An Investigation of a general Theorem for finding  
the Length of any Arc of a Conic Hyperbola, by Means  
of Two Elliptic Arcs, with some other new and useful  
Theorems deduced therefrom.* By John Landen, F. R. S.  
p. 283.
- XXVII. *Observations made at Chislehurst, in Kent, in the  
Year 1774.* By the Rev. Francis Wollaston, LL.B.  
F. R. S. p. 290
- XXVIII. *Of Triangles described in Circles and about them.*  
By John Stedman, M. D. p. 296
- XXIX. *De Polygonis Area vel Perimetro maximis et mini-  
mis, inscriptis Circulo, vel Circulum circumscriptis.*  
Auctore S. Horsley, LL. D. R. S. Sec. p. 301
- XXX. *An*

XXX. *An Account of an extraordinary acephalous Birth.*  
By W. Cooper, M. D. in a Letter to William Hunter,  
M. D. F. R. S. P. 311

XXXI. *Observations on the State of Population in Manchester, and other adjacent Places, concluded.* By Thomas Percival, M. D. F. R. S. and S. A. Communicated  
by the Rev. Dr. Price, F. R. S. P. 322

XXXII. *An Account of the Effects of Lightning on a House, which was furnished with a pointed Conductor, at Tenderden, in Kent.* In Two Letters from Richard Haffenden, Esquire, the Proprietor of the House, to Mr. Henley. To which are added some Remarks by Mr. Henley. P. 336

XXXIII. *Of the Torpidity of Swallows and Martins.* By James Cornish, Surgeon, at Totness, Devonshire, in sundry Letters to the Honourable Daines Barrington, F. R. S. and M. Maty, M. D. Sec. R. S. P. 343

XXXIV. *Description and Use of a portable Wind Gage.*  
By Dr. James Lind, Physician, at Edinburgh. P. 353

XXXV. *Astronomical Observations made at Leicester.*  
By the Rev. Mr. Ludlam, Vicar of Norton, near Leicester. Communicated by the Astronomer Royal. P. 366

XXXVI. *Remarks and Considerations relative to the Performance of Amputation above the Knee, by the single circular Incision.* By Benjamin Gooch, Surgeon at Norwich. P. 373

XXXVII. *Concerning Aneurysms in the Thigh.* By Benjamin Gooch, Surgeon at Norwich. P. 378

XXXVIII. *An*

- XXXVIII. *An Account of further Discoveries in Air.* By the Rev. Joseph Priestley, LL.D. F. R. S. in Letters to Sir John Pringle, Bart. P. R. S. and the Rev. Dr. Price, F. R. S. P. 384
- XXXIX. *An Account of the Gymnotus Electricus.* By John Hunter, F. R. S. P. 395
- XL. *Some Observations upon Myrrh, made in Abyffinia, in the Year 1771, and sent to William Hunter, M. D. with Specimens, in February, 1775.* By James Bruce, Esq. P. 408
- XLI. *An Account of a curious Giant's Causeway, or Group of angular Columns, newly discovered in the Euganean Hills, near Padua, in Italy. In a Letter from John Strange, Esq. F. R. S. to Sir John Pringle, Bart. P. R. S.* P. 418
- XLII. *Observations on the Difference between the Duration of Human Life in Towns and in Country Parishes and Villages.* By the Rev. Richard Price, D. D. F. R. S. Communicated by Dr. Horsley. P. 424
- XLIII. *Experiments on Animals and Vegetables, with respect to the Power of producing Heat.* By John Hunter, F. R. S. P. 446
- XLIV. *A Comparison of the Heat of London and Edinburgh.* By John Roebuck, M. D. F. R. S. in a Letter to William Heberden, M. D. F. R. S. P. 459
- XLV. *Experiments in an heated Room.* By Matthew Dobson, M. D. In a Letter to John Fothergill, M. D. F. R. S. P. 463
- XLVI. *Cal-*

# C O N T E N T S.

vii

- XLVI. *Calculations in Spherical Trigonometry abridged.*  
*By Israel Lyons. In a Letter to Sir John Pringle,*  
*Bart. P. R. S.* p. 470
- XLVII. *Further Experiments and Observations in an heated*  
*Room. By Charles Blagden, M. D. F. R. S.* p. 484
- XLVIII. *A Proposal for measuring the Attraction of some*  
*Hill in this Kingdom by Astronomical Observations. By*  
*the Rev. Nevil Maskelyne, B. D. F. R. S. and Astrono-*  
*mer Royal.* p. 494
- XLIX. *An Account of Observations made on the Moun-*  
*tain Schehallien for finding its Attraction. By the Rev.*  
*Nevil Maskelyne, B. D. F. R. S. and Astronomer Royal.*  
p. 500



---



---

P H I L O S O P H I C A L  
T R A N S A C T I O N S.

XVI. *An abridged State of the Weather at London in the Year 1774, collected from the Meteorological Journal of the Royal Society. By S. Horfley, LL.D. Sec. R. S.*

ALTHOUGH the practice of keeping meteorological journals is, of late years, become very general, no information of any importance hath yet been derived from it. The reason of which perhaps may be, that after great pains and attention bestowed in registering particulars, as they occur, with a scrupulous minuteness, observers have not taken the trouble to form, at proper intervals of time, compendious abstracts of their records, exhibiting the general result of their observations in each distinct branch of meteorology. The following tables are given as an example of the method that may be taken in future to remedy this neglect. With the general state of the barometer and thermometer, already given at the end of the meteorological journal, they form a history of the weather at London during the last year. If the example were to be followed, in different parts of the kingdom, we might in time be furnished with an experimental history of the weather of our island.

T A B L E I.

An abridged View of the WINDS at LONDON, in the Year 1774, Compiled from the Meteorological Journal of the Royal Society.												
	N	S	E	W	NW	SE	NE	SW		Rain.		
January	1½	0	2	1½	4	2½	4	13	31	2,958	{ Five half-days omitted in the Journal.	
February	1	1½	1	3	2	0	3½	16	28	2,360		
March	2	1½	1	1	½	3	14	7½	31	1,780	{ Half of a day missed in the Journal.	
April	2½	3	2	4½	2½	2	5	8½	30	1,242		
May	3	½	3½	0	6	3	10½	4	31	1,413	{ An half-day missed in the Journal.	
June	½	3½	2½	2½	4	2	1½	13½	30	2,273		
July	½	2	0	1	6½	2	1	18	31	2,438		
August	2	1½	0	1½	2	4	6	14	31	3,340		
Septemb.	1½	2½	1	2½	4	3½	6	9	30	3,917		
October	1	2½	1	2	3½	3	8	10	31	1,215		
Novemb.	7	1	1½	0	2½	1½	7	9½	30	1,586		
December	2½	1½	2	4½	6	3½	7½	3½	31	1,806		
	25	21	17½	24	43½	30	74	126½		26,328		

This table shews the number of days that each wind blew in each month, dividing the compass only into eight points, and reckoning all the winds between N. and W., N.W.; all between S. and E., S.E.; all between N. and E., N.E.; and all between S. and W., S.W. The number of days that each blew in all the months being collected into one sum at bottom, shews the number of days each wind blew in the whole year. The quantity of rain that fell in each month is added, that the connection between wet and dry, and the several winds may

the more readily appear. It appears that the winds from the S.W. prevailed more than any other in the year 1774; and next to the S.W. the N.E. But the S.W. was more frequent than the N.E. in the proportion of 126 to 74. Of the winds from the four cardinal points the North was the most frequent, and the East the most rare. In the three summer months, June, July, and August, there fell more rain than in the three of any other season. Of the 26,328 inches of rain which fell in the whole year, 13,842 fell in the winter half-year, consisting of the six months of September, October, November, December, January, and February, and 12,486 in the summer half-year, consisting of the six months of March, April, May, June, July, and August. So that

the winter's rain exceeded the summer's by 1,356; that is, by little more than  $\frac{1}{10}$ th part of half the rain of the whole year. September gave the greatest quantity of rain, and October the least of any single month in the whole year.

In collecting the rain of the several months, my rule, with respect to what hath sometimes fallen in the night between the last day of one month and the first of the next following, hath been this. When it appears by the journal, that it was fair on the last day of the month, at the time of the afternoon observations, I have given the whole of the ensuing night's rain to the new month; but if it rained on the last day of the month, at the time of the afternoon observation, I have divided the night's



rain equally between the new month and the old one. For instance, it appears by the journal that 0,043 fell in the night between the last day of February and the first of March. The whole of this I have placed to the account of March; because it was fair at the time of the afternoon observation on the last day of February. Again, in the night between the last day of September and the first of October, there fell 0,347. Half of this I give to September's rain and half to October's; because it rained the last day of September at the time of the afternoon observation.

TABLE II.

Sub-division of the S.W.				
	WSW	SW	SSW	
January	2	9½	1½	13
February	4	7½	4½	16
March	1½	4½	1½	7½
April	3	3	2½	8½
May	½	2	1½	4
June	1	9	3½	13½
July	5	9	4	18
August	5½	4½	4	14
September	2	1½	5½	9
October	2	5	3	10
November	2½	5	2	9½
December	1	2	½	3½
	30.	62½	34.	126½

TABLE III.

Sub-division of the N.E.				
	ENE	NE	NNE	
January	2½	½	1	4
February	0	2½	1	3½
March	2½	9	2½	14
April	½	3½	1	5
May	2	8½	0	10½
June	0	½	1	1½
July	0	½	½	1
August	2	1	3	6
September	1	1	4	6
October	2½	3½	2	8
November	4	1½	1½	7
December	1½	3½	2½	7½
	18½	35½	20	74

In these two tables the winds between the W. and the S.W. are all set down to the W.S.W.; and those between the S. and the S.W. are all reckoned S.S.W. In like manner,

ner, the winds between the E. and N.E. are all reckoned E.N.E.; and those between the N. and N.E. are all reckoned N.N.E. It appears that of the winds between the S. and W. those from the point of S.W. were far more frequent than those from either side of it. And the winds from the point of N. E. more frequent than those on either side of it, nearly in the same proportion.

TABLE IV.

Sub-division of the S.E.				
	ES	SE	SSE	
January	$1\frac{1}{2}$	1	0	$2\frac{1}{2}$
February	0	0	0	0
March	$\frac{1}{2}$	$1\frac{1}{2}$	1	3
April	0	1	1	2
May	0	0	3	3
June	0	$1\frac{1}{2}$	$1\frac{1}{2}$	2
July	$\frac{1}{2}$	0	$1\frac{1}{2}$	2
August	1	$2\frac{1}{2}$	$1\frac{1}{2}$	4
September	0	$1\frac{1}{2}$	2	$3\frac{1}{2}$
October	$\frac{1}{2}$	$1\frac{1}{2}$	1	3
November	0	1	$\frac{1}{2}$	$1\frac{1}{2}$
December	0	$2\frac{1}{2}$	1	$3\frac{1}{2}$
	4	14	12	30

TABLE V.

Sub-division of the N.W.				
	WNW	NW	NNW	
January	$\frac{1}{2}$	$2\frac{1}{2}$	1	4
February	1	1	0	2
March	0	$\frac{1}{2}$	0	$\frac{1}{2}$
April	$\frac{1}{2}$	$1\frac{1}{2}$	$\frac{1}{2}$	$2\frac{1}{2}$
May	$\frac{1}{2}$	$3\frac{1}{2}$	2	6
June	$1\frac{1}{2}$	$1\frac{1}{2}$	1	4
July	2	$4\frac{1}{2}$	0	$6\frac{1}{2}$
August	0	$1\frac{1}{2}$	$\frac{1}{2}$	2
September	2	1	1	4
October	0	$3\frac{1}{2}$	0	$3\frac{1}{2}$
November	1	$\frac{1}{2}$	1	$2\frac{1}{2}$
December	$2\frac{1}{2}$	$3\frac{1}{2}$	0	6
	$11\frac{1}{2}$	25	7	$43\frac{1}{2}$

By these two tables it appears, that of all the winds between the N. and W. those from the point of N.W. were far more frequent than those from either side of it. Of the winds between the S. and E. those from the point of S.E. were more frequent than those to the E. of that point, and rather more frequent than those to the S. of it.

it; but the difference in the latter case was very inconsiderable. Of the winds from all quarters, those from the E.S.E and N.N.W. were the most rare, especially the former. The numbers in the last columns of each of the four last tables are the sums of the preceding columns ranging in the same horizontal lines. They ought to correspond with the numbers in columns S.W. N.E. S.E. N.W. of TABLE I. respectively, and serve as a check upon the work in making the tables.

The general state of the winds collected from the five preceding tables, according to their different degrees of prevalence, is as follows:

ESE	NNW	WNW	SSE	SE	E	ENE	NNE	S	W	NW	N	WSW	SSW	NE	SW	
4	7	11½	12	14	17½	18½	20	21	24	25	25	30	34	35½	62½	361½

Days missed in the Journal,       -       -       -       -       -       -       3½

365

TABLE VI.

Shewing the number of fair and frosty days in each half month and in the whole year.

	Fair		Fair days in whole month.	Frosty days		Frosty days in whole month.
	1st half.	Latter half.		1st half.	Latter half.	
January	9.	6.	15	10	7	17
February	7	3.	10	6		6
March	7	13	20	4		4
April	11	8.	19			
May	8	6.	14			
June	10	6	16.			
July	6	8.	14.			
August	9	8.	17.			
September	7	2	9			
October	12	10	22			
Novemb.	5.	6	11		1	1
December	6	13	19	5	3	8
Total fair days,			186	Total frost,		36

There were but 10 days in the whole year that gave any snow; viz.

3 in January, 1 in February, 5 in November, and 1 in December. The first snow on the 9th of January in the afternoon, after a rainy morning, set in with a N.N.E. wind, and was succeeded by a sharp frost.

for three days and a half, with the wind E.N.E. The second, which happened in the night between the seventeenth and eighteenth, came likewise after rain, and was succeeded by a frost of four days and a half, wind shifting between N.W. and S.E. The last snow in January, on the 24th, fell with a S.W. wind, which set in the day before. It was followed by a moderate frost of

one.

one day, though the wind continued in the S.W. The snow on the 1st of February came with a S.W. during a sharp frost. The wind was in the N.E. before the snow, and returned to the same point the next morning; the frost sharper than before the snow. The snows in the latter part of November were generally accompanied with rain, and did not bring actual frost. The snow on the 9th of December came after two days frost, which it seems to have put an end to. For though it froze in the evening after the snow, the frost was much less severe than the preceding night, and a thaw came with rain, wind N.E., the next day.

There were only two thunder storms this year, *videlicet*,

August 27. 2 P.M. Barometer 29,64 inches, Thermometer 63°, Wind N.W.

September 24. 9 P.M. Barometer 29,42 inches, Thermometer at 2 P.M. 64°.

## T A B L E VII.

For Trial of the Influence of the WINDS upon the BAROMETER.

	SW	WSW	SSW	NE	ENE	NNE	SE	ESE	SSE	NW	WNW	NNW	N	S	E	W
Jan.	29,37	29,61	29,60	29,34	29,64	29,58	29,57	29,72		29,66	29,895	29,675	29,72		29,80	29,67
Feb.	29,63	29,81	29,70	30,30		30,41				29,73	29,615		29,69	29,86	29,85	29,90
Mar.	29,39	29,79	29,89	29,945	29,87	30,20	29,52	29,88	29,52	29,615			30,14	29,87	29,90	29,75
April	29,77	30,07	29,61	29,95	30,21	29,80	30,00		29,69	29,80	29,58	29,90	29,82	29,475	29,53	29,80
May	29,91	29,46	29,45	29,96	29,86				29,71	29,81	29,97	30,04	29,98	29,34	29,925	29,80
June	29,91	29,92	29,80	30,21		29,75	29,92		29,95	30,00	29,69	29,87	29,60	29,83	29,84	30,09
July	29,97	29,97	29,98	29,92		30,33		29,78	29,97	30,07	30,085		30,26	30,25		29,94
Aug.	29,97	29,85	29,80	29,95	30,00	30,17	30,05	30,07	29,535	29,960		30,12	30,13	29,74		29,98
Sept.	29,89	29,80	29,70	29,97	29,82	29,94	29,51		29,56	29,925	29,82	29,94	29,85	29,64	29,775	29,76
Oct.	29,98	30,28	30,225	30,32	29,865	30,23	30,15	29,54	30,18	29,98			30,19	30,22	29,95	30,27
Nov.	29,74	29,91	29,92	29,79	29,73	29,53	29,84		29,86	29,81	29,74	29,90	29,93	29,61	29,54	30,51
Dec.	30,07	29,795	29,71	30,15	29,82	30,38	29,62		29,20	30,21	30,31		30,38	29,64	29,90	30,51
Means	29,76	29,92	29,80	30,02	29,82	30,01	29,80	29,81	29,70	29,94	29,93	29,92	29,99	29,80	29,80	30,02

It is an old observation, that a N.E. wind in this country generally makes the barometer rise. This naturally leads to an enquiry, whether there be any general connection of the rise and fall of the barometer with the setting of the wind. Upon comparing the general account of the barometer for the year 1774, as stated at the end of the meteorological journal, with the journal at large, I found, that in seven months out of the twelve the greatest height of the barometer was accompanied with a North-easterly wind; and in eight months out of the twelve, the least height of the barometer was accompanied with a S.W. This incited me to take the trouble of making out the preceding table, which shews the mean height of the barometer which accompanied each wind in every month, and for the whole year. And it appears, that though the barometer may be almost at any height with any wind, yet the mean height was greater, in the course of the last year, with the winds which set from that semicircle of the compass, which is intercepted between the points of W.S.W. inclusive and E.N.E. exclusive, going round by the W. and N. than with the winds which set from the opposite semi-circle intercepted between E.N.E. inclusive and W.S.W. exclusive, going round by E. and S. In the former semi-circle the W. and N.E. give the greatest mean height, and in the latter the S.S.E. and S.W. give the least\*.

\* It is to be noted, that the means of the whole year, stated in the lowermost horizontal row, are not found by collecting the means of all the months into one sum, and dividing by the number of months (for this method would always be fallacious, except each wind had blown for the same number of days in all the different months); but by adding together the heights attending each wind day by day, and dividing the sum by the number of days each wind blew in the whole year.

T A B L E VIII.

For Trial of the MOON'S Influence.

	Last Qr.	New.	First Qr.	Foll.	
	D. H.	D. H.	D. H.	D. H.	
Jan.	5 6	11 21	19 3	27 7	$\begin{array}{ccccccc} \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} * & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} \overset{\sim}{\ominus} \\ \overset{\sim}{+} & \overset{\sim}{-} & & \overset{\sim}{-} & \overset{\sim}{-} & \overset{\sim}{-} & \overset{\sim}{-} \overset{\sim}{\ominus} \\ 6 & 7 & 9 & 10 & 14 & 18 & 23 \end{array}$ 25 26 30
Feb.	3 15	10 9	18 0	25 23	$\begin{array}{ccccccc} + - & & & & & & \\ \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} \overset{\sim}{\ominus} \\ 4 & 7 & 8 & 10 & 13 & 15 & 17 \end{array}$ 20
Mar.	4 22	11 22	19 20	27 11	$\begin{array}{ccccccc} \overset{\sim}{\ominus} & & & & & & \\ \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} \overset{\sim}{\ominus} \\ 10 & & & & & & \end{array}$
Apr.	3 5	10 12	18 15	25 22	$\begin{array}{ccccccc} \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} \overset{\sim}{\ominus} \\ 4 & 8 & 28 & 29 & & & \end{array}$
May	2 12	10 3	18 7	25 5	$\begin{array}{ccccccc} \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} \overset{\sim}{\ominus} \\ 5 & 7 & 23 & & & & \end{array}$ Last Qr. 31 20
June	8 18	16 19	23 12	30 7	$\begin{array}{ccccccc} \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} \\ 6 & 17 & 18 & 20 & 22 & 28 & 30 \end{array}$
July	8 9	16 5	22 19	29 20	$\begin{array}{ccccccc} \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} \overset{\sim}{\ominus} \\ 5 & 15 & 20 & 22 & 27 & 31 & \end{array}$
Aug.	7 0	14 12	21 3	28 12	$\begin{array}{ccccccc} \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} \overset{\sim}{\ominus} \\ 4 & 6 & 11 & 15 & 17 & 26 & \end{array}$
Sept.	5 14	12 17	19 13	27 7	$\begin{array}{ccccccc} \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} \\ 1 & 5 & 7 & 11 & 13 & 14 & 17 \end{array}$ 20 22
Oct.	5 3	12 0	19 2	27 3	$\begin{array}{ccccccc} \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} \overset{\sim}{\ominus} \\ 3 & 23 & 24 & 29 & & & \end{array}$
Nov.	3 15	10 7	17 18	25 23	$\begin{array}{ccccccc} \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} \\ 2 & 6 & 7 & 18 & 21 & 26 & \end{array}$
Dec.	3 2	9 17	17 12	25 17	$\begin{array}{ccccccc} \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} & \overset{\sim}{\ominus} & \overset{\sim}{+} \overset{\sim}{\ominus} \\ 2 & 5 & 11 & 14 & 15 & & \end{array}$



This table exhibits a comparison of the actual changes of the weather from fair to foul, with the aspects of the Moon; and needs no other explanation than an interpretation of the characters in the last column.

— frost	} Any one of these marks placed over a number signifies, that the weather indicated by that mark continued from the day of the month denoted by the number underneath to the day denoted by the next following number, bearing some other mark over it.
+ thaw	
○ fair	
~ rainy	
~ stormy	
* snow	

Thus, in the month of July, rainy weather set in on the fifth, and lasted to the fifteenth; from the 15th to the 20th it was fine; when it changed again, and continued rainy till the 22d; then it was fine to the 27th, and rainy again till the 31st.

Such tables of comparison, made yearly for a succession of years, would in the end decide with certainty for or against the popular persuasion of the Moon's influence upon the changes of our weather; which hath some how or other gained credit even among the learned, without that strict empiric examination, which a notion in itself so improbable, so destitute of all foundation in physical theory, so little supported by any plausible analogy, ought to undergo. The vulgar doctrine about this influence is, that it is exerted at the syzygies and quadratures, and for three days before and after each of those epochs. There are 24 days therefore in each synodic month, over which the Moon at this rate is supposed to preside; and as the whole consists but of 29 days  $12\frac{3}{4}$  hours,

hours, only  $5\frac{1}{2}$  days are exempt from her pretended dominion. Hence, though the changes of the weather should happen to have no connection whatever with the Moon's aspects, though the fact should be, that they take place at all times of the Moon indifferently, and are distributed in an equal proportion through the whole synodic month; yet any one who shall predict, that a change shall happen on some one of the 24 days assigned, rather than on any one of the remaining  $5\frac{1}{2}$ , will always have the chances 24 to  $5\frac{1}{2}$  in his favour. Merely because more changes will fall within the greater time, and, upon an average, as many more in proportion as the time is greater. It is evident therefore, that this is a matter in which men may easily deceive themselves, especially in so unsettled a climate as that of this island: and the advocates for lunar influence are not to imagine they have fact on their side, unless it should appear, from such tables as these carefully kept for a long course of years, that the changes happening on the days, which they hold to be subject to the Moon, are more than those which happen on the exempted days, in a much greater proportion than that of 24 to  $5\frac{1}{2}$ .

The antiquity of the opinion may perhaps be allowed in its favour; and it may seem an answer to the objection taken from the instability of the weather of this part of the world, that it had its origin in more settled climates. We find it, it must be confessed, in the earliest Greek writers, who probably had it with the rest of their physics from the East. And to this circumstance, I

are persuaded, the opinion owes the credit it hath met with among men of learning. But whatever general assertions may be found in some writers, concerning celestial influences in general, and the Moon's in particular, as being of all the heavenly bodies the nearest to the earth, the writers who treat of the signs of the weather practically, for the information of husbandmen and mariners, derive their prognostics from circumstances, which neither argue any real influence of the Moon as a cause, nor any belief of such an influence; but are merely indications of the state of the air at the time of observation: namely, the shape of the horns, the degree and colour of the light, and the number and quality of the luminous circles which sometimes surround the Moon, and the circumstances attending their disappearance<sup>(a)</sup>. It is true, that each of these prognostics is expressly confined, by the early writers, to a particular time of the Moon's age<sup>(b)</sup>. But not, as I conceive, on account of any particular influence of the Moon in this or that aspect; but merely because the prognostics, that she affords at one age, are such in themselves as she cannot afford at another. For instance, the bluntness of the horns in the new Moon is a sign of approaching rain,

(a) See the *Διοσημεια* of Aratus and the Scholia of Theon.

(b) *Σημάδα δ' ἔσ' ἂρ' πᾶσιν ἐπ' ἡμασι πάντα τέτυκται.*

Ἄλλ' ὅσα μὲν τριτάτῃ τετρατάτῃ τε πέλῃσαι,

Μίσφα διχαιομένης· διχάδος γέ μιν ἄχρις ἐπ' αὐτὴν

Σημαίνει διχόμηνον· ἀτὰρ πάλιν ἐκ διχομήνης

Ἐς διχάδα φθιμένην· ἔχειται δέ οἱ αὐτίκα τετρὰς

Μηνὶς ἀποικομένη.

Ἀρατ. *Διοσημεια*.

because

because it indicates a turbid state of the atmosphere; for if the air were clear and dry, the horns should appear sharp and pointed, that being then their natural shape. But the bluntness of the horns is no sign of change after the dichotomy; because then the horns will appear blunt in all states of the air, the elliptic arc on the deficient side of the Moon presenting its concavity to the circular limb, and forming with it an obtuse angle. Again, the degree of the Moon's light on the fourth day furnished a prognostic. It ought then to be strong enough, if the air was clear, for terrestrial objects to cast a shadow<sup>(c)</sup>. If their shadows were not discernible, it was a sign that the air was impure, and bad weather was to be expected. But this prognostic did not take place *before* the fourth day, because the light of the Moon was yet too weak for shadows to be formed in the purest state of the air. It did not take place *after* the fourth day, because the enlightened part was then so much encreased, that shadows would be formed in any state of the air, if the Moon was not actually hidden by a cloud, or obscured by sensible mists. The prognostics furnished by the new Moon served only till the dichotomy, and those of the dichotomy till the full Moon, and so on; not because a new and distinct influence was exerted in each new aspect, but because each new aspect furnished a new set of signs, of a

(c) — ὅτε πρώτη ἀποσκιάσθαι αὐτὸθεν αὐγὴ

Ὅσσον ἐπισκιάειν ἐπὶ τέττατον ἡμέραν ἴσσω. Αἰγ. Διοσημεια.

Τετταρία γενόμενη ἡ σελήνη ἀρχεῖται δύνασθαι σκιάζειν ἐν τῇ φάτι αὐτῆς· τριταῖο γὰρ εἰ δύναται διὰ τὴν περικειμένην τῇ φάτι ἀδράνειαν. Theon in locum.

different kind. That this is a true representation of the most ancient lunar prognostics, appears from hence; that others of a similar kind were derived from the Sun and the fixed stars, particularly the *Præsepe* and *Aselli* in Cancer, and the bright star in the Altar.; and it is remarkable, that ARATUS says, the prognostics taken from the Sun are the most certain of all (*d*). The vulgar soon began to consider those things as causes, which had been proposed to them only as signs. The manifest effect of the Moon upon the Ocean, while the mechanical cause of it was totally unknown, was interpreted as an argument of her influence over all terrestrial things; and these notions were so consistent with that visionary philosophy, which assigned distinct places to corruption, change, and passivity, on the one side, and the active governing powers of nature on the other, and made the orb of the Moon the boundary between the two, that they who should have been its opponents, ranged themselves on the side of popular prejudice. And the uncertain conclusions of an ill-conducted analogy, and a false metaphysic, were mixed with the few simple precepts derived from observation, which probably made the whole of the science of prognostication in its earliest and purest state. Hence both THEOPHRASTUS and ARATUS teach us to remark the position of the Moon's horns, and take conjectures of approaching fair weather or tempest, according as they appear, at different times of the Moon's age, erect, reclined, or prone: not knowing

(d) *Ἡελίος ἔμᾶλλον ἰσχυρότα σήματα κεῖται. Διοσημεία.*

that the position of the line joining the Moon's cusps, with respect to the horizon, depends merely upon the mutual approach, or recess, of the pole of a great circle drawn through the centers of the Sun and Moon, and the pole of the horizon, in the course of the diurnal revolution. And so great a man as VARRO, as he is quoted by PLINY, was not ashamed to give this childish rule, for predicting the weather, for a whole month to come, from appearances at the new Moon. "If the upper horn " be obscure, the decline of the Moon will bring rain. " If the lower horn, the rain will happen before the full. " At the time of the full Moon, if the blackness be in " the middle<sup>(e)</sup>." After this one cannot be surprized, that the poet VIRGIL should make the prognostics of the fourth day decisive for the whole lunation:

*Sin ortu quarto, namque is certissimus auctor,  
Pura neque obtusis per cælum cornibus ibit,  
Totus et ille dies, et qui nascentur ab illo,  
Exactum ad mensem pluvia ventisque carebunt.*

Georgic. lib. 1. lin. 143.

But in this he contradicts ARATUS, whose authority in general he follows implicitly. With ARATUS, the signs of the new Moon extend only to the first quarter.

The ancients ascribed an influence to the constellations and fixed stars as well as to the Sun and Moon; and

(e) Apud Varronem ita est. ——— Nascens Luna si cornu superiore obatro furget, pluvias decrescens dabit: si inferiore, ante plenilunium: si in media nigritia illa fuerit, imbrem in pleno. PLIN. Nat. Hist. lib. XVIII. cap. 35.

there seems to have been much the same foundation for one as the other. In the *paraepgmata* or calendars, introduced in Greece, as we learn from THEON (*o*), by the astronomer METON, and renewed either annually or, as I rather conjecture, at the expiration of every 19-year period, the heliacal risings and settings of different stars were marked as bringing in different sorts of weather. The truth is, the earliest astronomers imagined, that the weather was governed by the Sun; and that its varieties were every where owing to the different degrees of the Sun's heat in the different seasons. They had therefore taken great pains to collect, by a long series of observations, the weather that usually prevailed in this or that particular place during the Sun's passage through every degree of every sign. Upon these observations, not upon any whimsical theory of celestial influences, the predictions in the calendars were founded. It seemed reasonable to announce, as the weather of each part of the year, what had been found to be then most frequent. And while the civil reckonings of time were so different among the different Greek states,\* and so rudely digested in all, the heliacal risings and settings of the stars were the only certain and obvious marks, the compilers of those popular directories could hit upon, of the Sun's return to the different parts of the zodiac(*s*). Hence they proposed them

(*f*) Scholia in Aratum.

(*g*) Geminus. Εἰσαγωγή εἰς τὰ φαινόμενα. c. 14.

to the people as signals of the weather to be expected. The form of the year being now the same in all parts of Europe, and pretty accurately adjusted to the motions of the heavenly bodies, and the heliacal risings and settings of the stars, from the different manner of life of our country people, not falling so much under popular observation with us, as they did among the Greeks, they are not marked as prognostics in our modern almanacks: and this I take to be the reason, that though the Moon hath maintained her reputation amongst us, the influence of the fixed stars is sunk, as it well deserves, in utter oblivion. Upon the whole I do not deny, that the observant husbandman will find a variety of useful prognostics in the appearances of the Moon, and the heavenly bodies in general; but they will be prognostics of no other kind, and for no other reason (though perhaps less fallible) than the sputtering of the oil in the industrious maiden's lamp, or the excrescences which gather round the wick<sup>(b)</sup>. They will be symptoms destitute of all efficient powers. They will shew the present state of the air, as that on which they depend, not as that which they

(b) *Ne nocturna quidem carpentes pensa puellæ  
Nescivere hiemem: testâ cum ardente viderent  
Scintillare oleum, et putris concrefcere fungos.*

*Georgic. lib. I. lin. 395.*

Ἡ λύχνῳ μύκητες ἀγείρωνται περὶ ρούξαν,  
Νύκτα καὶ σκολίην, μηδ' ἦν ὑπὸ χεϊμάτος ὤρη  
Λύχων ἄλλοι μὲν τε φάος καὶ κόσμον ὁρώμεν,

\* Ἄλλοι δ' αἴσσωσιν ἀπὸ φλόγης, ἥντε κῶφαι

Πομφόλυγες &c.

Αρτ. Διοσκρ.



govern, and may furnish probable conjectures for two or three days to come. To what I have already advanced in support of this opinion, I shall only add the last lines of the *Διοσημεία* of ARATUS. They speak the sentiments of the earliest ages most decisively, as they shew how little the doctrine of the influence of lunar aspect had gained ground, even in his days, among practical writers. That elegant versifier, there is little room to doubt, delivers the practical maxims of his time, just as he received them. He was too little of a poet to disguise the truth with ornamental fiction, and too little of a philosopher to adulterate it with hypothesis.

Τῶν μηδὲν καλόκησο, καλὸν δ' ἐπὶ σήματι σῆμα  
 Σκέπτεσθαι, μᾶλλον δὲ δυοῖν εἰς ταυτὸν ἰόντων  
 Ἐλπωρὴ τελέθοι· τριάτῳ δέ κε θαρσησεΐας.  
 Αἰεὶ δ' ἂν περιούλιος ἀριθμοῖς ἐνιαυτῷ  
 Σήμαλα, συμβάλλων εἶπε καὶ ἐπ' ἀσέρι τοίῳ  
 Ὡς ἀνέλλουσι κατέρχεται, ἢ καλιόνει,  
 Ὅπποῖον καὶ σῆμα λέγοι· μάλα δ' ἄρκιον εἶη  
 Φράζεσθαι φθίνοντος, ἐφισταμένοιο τε μηνὸς  
 Τετράδας ἀμφοτέρας· αἱ γάρ τ' ἄμυδις συνιόντων  
 Μηνῶν πειρατὶ ἔχουσιν, ὅτε σφαλερώτατος αἰθήρ  
 Ὡκῶ νυξὶ πέλει, χήτεϊ χαροποῖο σελήνης.  
 Τῶν ἀμυδις πάντων ἐσκεμμένος εἰς ἐνιαυτὸν,  
 Οὐδέποτε σχεδὺς κεν ἐπ' αἰθέρι τεκμήραιο.

Which

Which I render thus : “ Neglect none of these prognostics [none, he means, of the great variety he hath enumerated, taken from the heavens, from animals, plants, terrestrial objects, &c.], it is a good thing to combine the observation of one prognostic with another. If two agree, there is the greater likelihood of the event, and a third makes it certain. Whatever you do, register [*ἀριθμῶνς*] the prognostics of the current year, carefully noting what the prognostic says [*ὁπποῖον ἢ σῆμα λέγοι*; that is, what the event shew it to be a sign of], if such a sort of morning <sup>(i)</sup> comes on with the rise or setting of any particular star. And it will be of the

(i) *Such a sort of morning.* That is, a morning marked with such or such appearances. So I understand *τείν η̄ας*. The spirit of the precept seems to be, that the heliacal risings of the stars are to be attended to, in conjunction with the particular appearances attending the dawn or sun-rise. The heliacal risings shew the season, or general constitution of the time of the year; the particular appearances of the morning indicate the minute circumstances of the weather for two or three days to come. Thus the heliacal rising of Arcturus was a sign, in all the ancient parapegms, that the stormy season was at hand, and bad weather of various sorts, rain, thunder, high wind, was to be expected; but what the particular weather would be for a day or two to come, whether it would be only windy, or wet, with thunder or without, from what quarter the bad weather would come, all this would be pre-signified by the particular appearances of the morning. Perhaps the same appearance may be subject to some variety of interpretation at different seasons of the year, and in different places. In this, experience and observation will be the only sure guides. And for this reason ARATUS advises his scholar, not only to attend to the general rules laid down for him, but to keep a journal for himself, and make his own conclusions.

“ highest importance to attend particularly to the two  
 “ quaternions of the expiring and the incipient month<sup>(k)</sup>  
 “ [that is to the four last days of the month going out,  
 “ and the four first of that which is setting in], for they  
 “ comprise the extremities of the two months, where  
 “ they meet : and the weather [or the state of the air]  
 “ is then particularly uncertain [difficult to guess at] for  
 “ eight nights, for want of the silver-coloured Moon.  
 “ If you attend to all these put together, all through the  
 “ year, you will never form a random guess about the  
 “ weather.” The uncertainty of the weather for these  
 eight nights cannot be an uncertainty of the effect depending upon the moon’s aspect; but it is an uncertainty of fore-knowledge, the poet speaks of, for want of the Moon as an index. For though the word *σφαλερώτατος* by itself would be ambiguous, as it might be taken either in the sense of *δυσόχαστος* or *εὐμετέλλητος*, the words *χίται χαρποῖο σελήνης* are decisive for the first interpretation. The moon exists during these eight months as at other times. There is no want of her therefore as a physical agent : the only want there can be, is the want of her

(k) *And it will be of the highest importance to attend to, &c. μάλα δ’ ἀρκούν ἐν φράζεσθαι.* I have sometimes thought these words might be rendered thus :  
 “ This will be of great importance [that is, this joint observation of the general  
 “ indications of season and of particular prognostics will be of great importance]  
 “ in order to form a conjecture about the two quaternions, &c.” This interpretation would make the most connected meaning for the whole passage ; but I do not recollect, nor can I find upon the strictest search, any instance, wherein the verb *φράζεσθαι* is used in the sense of *conjecturing*, or *forming a judgement or opinion about*.

appearance. It would be unpardonable not to mention, that so great an authority as that of THEOPHRASTUS is against the side of the question to which I incline. The doctrine of the influence of lunar aspect is expressly asserted in his Treatise on the Signs of Rain and Wind. He says, that the new Moon is generally a time of bad weather, because the light of the Moon is wanting<sup>(p)</sup>; and that the changes of weather generally fall upon the syzygies or quadratures. But this seems to have been merely an opinion founded upon an imaginary analogy between the epochs of syzygy and quadrature in the months, and the equinoxial and tropical epochs in the year. For the Moon, he says, is, as it were, the Sun of the night. THEOPHRASTUS, though a diligent observer of nature, was deep in the theory of that school, of which he was himself one of the brightest ornaments: and his testimony, with respect to the matter of fact, hath not, like ARATUS's, a credibility founded on the mediocrity of his genius.

In the table, p. 177. the changes which fell on the syzygies and quadratures, or on any one of PLINY's critical days of the Moon's age (which are the 3d, 7th, 11th, 15th, 19th, 23d, 27th,), are distinguished from the rest by a larger character<sup>(m)</sup>. And out of 69 changes register-

(l) Διὸ καὶ αἱ συνόδοι τῶν μηνῶν χρημέρισται· ὅτι ἀπολείπει τοῦ φωτὸς τῆς σελήνης, &c. THEOPHRAST. de signis Pluv. p. 417. Edit. Heinl.

(m) Sunt et ipsius Lunæ octo articuli quoties in angulos solis incidit, plerisque inter eos tantum observantibus præfagia ejus, hoc est tertia, septima, undecima, decima quinta, decima nona, vigesima tertia, vigesima septima, et inter-lunium. PLIN. Nat. Hist. lib. XVIII. c. 35.

ed in this table 32 claim that distinction. Which is rather a larger proportion of the whole number, than is due to the time made up of all the days of fyzygie and quadrature, in the whole year, together with PLINY's critical days, thrown into one sum. For since there were 365 days in the year, and the days of fyzygie and quadrature, with PLINY's critical days, amount to 113, out of 69 changes in the whole year 22 are as many as belong to these particular days, upon a proportional distribution. But in the preceding table, there are many alterations marked as changes, when it appears, that the weather returned to what it had been before the time of change, within the space of 24 hours after it. Now if we reject all these on both sides of the question (which I think is the fair way of reckoning, for sudden alterations, of so short a duration, are rather to be called irregularities than changes of weather), we shall find but 46 changes in all, from one settled state to another, of which only 20 fell on the days of fyzygies, quadrature, or PLINY's days, which is still more than the just proportion.

But again. PLINY's eight critical days were probably intended for the four days of fyzygie and quadrature and the four of octagonal aspect<sup>(n)</sup>. For if the time of the conjunction be rightly assumed, the mean quadratures, and the mean opposition, and the mean octagonal aspect, will always fall either on one of PLINY's days, or on the day next to it. The deviation, I sus-

(n) The words, Quoties in angulos solis incidit, imply this.

pect, was intentional, and for the sake of the odd numbers. Thus the 4th, 8th, and 12th days of the Moon should have been critical, instead of the 3d, 7th, and 11th, if the mean motions of the Moon had been the single thing attended to. But PLINY, or whoever was the first author of the rule he gives us, chose the latter as containing, besides much of the lunar influence, all the magic virtue of imparity, of which the others, taking their numerical denomination from even numbers, are totally destitute. Among the numerous believers in the Moon of our days, few, I suppose, retain any confidence in the physical powers of the odd numbers. They may imagine therefore, that the apparent inconsistency of PLINY's rule with the truth of things, may be owing to his superstition about the odd numbers, which led him wilfully to deviate from the mean epochs, little apprized (for the Romans never were astronomers) how much they sometimes differ from the true ones, on account of the great and various inequalities of the Moon's motions, and how very widely his arbitrary arrangement would in consequence often differ from the times it was intended nearly to represent. Instead of PLINY's critical days, I shall now, therefore, examine the days for which, I imagine, they were substituted; those I mean of true syzygie, true quadrature, and true octagonal aspect. The following table distinguishes the changes of weather which fell on these days. There were only 22 such out of all the 69; which is scarce four more than their even proportion. And rejecting, as before, on both sides,

the alterations of weather which were reversed within the space of 24 hours, there remain out of 46 changes in all only 10 upon the days of lunar influence, which are two less than belong to them upon the even chance; for the days of syzygie, quadrature, and octagonal aspect, in the whole year are 98; and  $365 : 98 = 46 : 12\frac{1}{2}$  very nearly. It is remarkable, that of these ten changes two only coincide with a new Moon; namely, those of the 10th of February and 5th of September, and none at all with a full Moon. There were indeed two changes in the year upon the day of the full Moon; *videlicet*, those of the 20th of September and 18th of November; but both were reversed within the space of 24 hours.

[ 193 ]

T A B L E IX.

January	6 7 9 10 14 18 23 25 26 30	10	3
February	4 7 8 10 13 15 17 20	8	3
March	10	1	0
April	4 8 28 29	4	1
May	5 7 23	3	0
June	6 17 18 20 22 28 30	7	3
July	5 15 20 22 27 31	6	1
August	4 6 11 15 17 26	6	2
September	1 5 7 11 13 14 17 20 22	9	4
October	3 23 24 29	4	1
November	2 6 7 18 21 26	6	3
December	2 5 11 14 15	5	1
		69	22

I have added in this table two columns, shewing the number of changes in each month, and the number out of each agreeing with the Moon.

I shall only add, that no conclusion must be drawn from the observations of a single year.



XVII. *Extract of a Meteorological Journal for the Year 1774, kept at Bristol, by Samuel Farr, M. D.*

Redde, March 23, 1775.

Months.	Barometer.				Rain.
	Highest	Lowest.	Mean.	Vicissitude.	
January	30,1	28,8	29,5	1 1-2	4,951
February	30,4	29,2	29,7	0 9-1	5,549
March	30,2	29,1	29,7	0 9-4	5,297
April	30,1	29,3	29,7	0 8-5	2,349
May	30,1	29,3	29,9	0 7-4	2,955
June	30,2	29,4	29,7	0 6-3	2,602
July	30,2	29,7	29,8	0 4-1	2,972
August	30,2	29,4	29,8	0 5-2	2,999
September	30,1	29,0	29,6	0 7-2	7,035
October	30,5	29,3	30,0	0 8-2	1,927
November	30,2	29,2	29,7	0 6-1	1,683
December	30,6	29,0	29,7½	0 7-2	2,047
					42,366

The barometer was placed seventeen yards above the level of the river Avon, which runs very near to my house. By vicissitude is meant the greatest rise or fall of the quicksilver in the smallest number of days.

S. FARR.

Dr.

Dr. FARR had also given the mean heights of the thermometer within doors for every month in the year. But these are omitted, because observations of the thermometer in the house are of no importance, unless accompanied with corresponding ones of an instrument kept in the shade in the open air. The air of a room, though kept without a fire, and so situated as never to see the Sun, alters its degree of heat or cold so much more slowly than the external air, that no judgement can be formed of the temperature of the one from that of the other: except after a continuance of weather of the same kind for a long time together, their mutual relation is vague and undetermined. Dr. FARR likewise sent a particular account of the winds and changes of the weather for every day of the year; from which I have composed the two following tables.

S. HORSLEY.

An abridged TABLE of the WINDS for BRISTOL,  
for the Year 1774.

	N	S	E	W	NW	SE	NE	SW		Number of Frosty Days.
January	3 $\frac{1}{2}$	$\frac{1}{2}$	6	3	1 $\frac{1}{2}$	2	7	7 $\frac{1}{2}$	31	10
February	$\frac{1}{2}$	1 $\frac{1}{2}$	$\frac{1}{2}$	1	3 $\frac{1}{2}$	3	5 $\frac{1}{2}$	11 $\frac{1}{2}$	27	7
March	$\frac{1}{2}$	1 $\frac{1}{2}$	4 $\frac{1}{2}$	$\frac{1}{2}$	3 $\frac{1}{2}$	5 $\frac{1}{2}$	11	4	31	7
April	$\frac{1}{2}$	2	$\frac{1}{2}$	0	8	4 $\frac{1}{2}$	5	8 $\frac{1}{2}$	29	
May	$\frac{1}{2}$	1 $\frac{1}{2}$	2	0	2	2	14 $\frac{1}{2}$	8 $\frac{1}{2}$	31	
June	1	2 $\frac{1}{2}$	2	$\frac{1}{2}$	4	1	2 $\frac{1}{2}$	16 $\frac{1}{2}$	30	
July	1	1	0	2	6 $\frac{1}{2}$	2	0	17 $\frac{1}{2}$	30	
August	0	$\frac{1}{2}$	1 $\frac{1}{2}$	0	1	4	6 $\frac{1}{2}$	17 $\frac{1}{2}$	31	
September	$\frac{1}{2}$	$\frac{1}{2}$	0	$\frac{1}{2}$	4	10	7 $\frac{1}{2}$	7	30	
October	0	1	2	$\frac{1}{2}$	3 $\frac{1}{2}$	6	5 $\frac{1}{2}$	12 $\frac{1}{2}$	31	Frost at times.
November	1	$\frac{1}{2}$	0	0	4	5	13 $\frac{1}{2}$	6	30	Frosty nights.
December	0	0	3	0	$\frac{1}{2}$	8	13 $\frac{1}{2}$	6	31	18
	9	13	22	8	42	53	92	123		42

3 days in the year are omitted in Dr. FARR's account;  
viz. Feb. 7. April 29. and July 12.

Thunder.

Thunder. February 16. 23. 24. S.W.

March 8. 20. E. 28. E. and W. 30. E. and N.E.

April 27. with hail storm, S.W. and N.W.

May 1. 4. N.E. 9. 10. E. 24. S.W. and S.E.

June 25. S.W. and S.

July 10. S.W. 26. N. and N.W.

September 4. S.E. and N.E. 6. N.W. 12. S.W. and S.E.

TABLE for Trial of the Moon's Influence at BRISTOL,  
for the Year 1774.

	Last Qr.	New.	1st Qr.	Full.			
	D. H.	D. H.	D. H.	D. H.	—	—	—
Jan.	5 6	11 21	19 3	27 7	1 5 10 13 19 22 30	7	2
Feb.	3 15	10 9	18 0	25 23	7	1	1
Mar.	4 22	11 22	19 20	27 11	6 10 17 20 30	5	1
Apr.	3 5	10 12	18 15	25 22	6 10 13	3	1
May	2 12	10 3	18 7	25 5	1 10 19	3	1
	New	1st Qr.	Full.	Last Qr.			
June	8 18	16 19	23 12	30 7	1 6 13	3	1
July	8 9	16 5	22 19	29 20	1 10 14 17 19 26	6	2
Aug.	7 0	14 12	21 3	28 12	1 3 7 25	4	3
Sept.	5 14	12 17	19 13	27 7	Only 9 fair days.		
Oct.	5 3	12 0	19 2	27 3	6 17 25 30 Cloudy with rain till the 5th. hard rain, then frequent frosty nights and gentle rain in the day-time.	4	0
Nov.	3 15	10 7	17 18	25 23			
Dec.	3 2	9 17	17 12	25 17	3 11 21	3	2
39 14							

When a number appears in this table without any character over it, it is to be understood, that the weather was quite unsettled from that day to the next bearing a mark; and when two or more marks are found over the same number, all the different kinds of weather, denoted by the several marks, took place on that day. The same is to be understood in the tables, p. 177. and p. 193.

This table distinguishes the changes of weather which fell on the days of true syzygie, true quadrature, and true octagonal aspect. Setting aside the very changeable months of September and November, there were 39 changes in the remaining 10, fourteen of which happened upon the days specified; which is almost 4 more than belong to them on the even chance. Of these 14 changes, only four fell upon the day of a new moon, and none at all upon the day of the full.

XVIII. *Extract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1774. By Thomas Barker, Esquire. Communicated by Sir John Pringle, Bart. P. R. S.*

Redde, March 23, 1775.

		Barometer.			Thermometer.						Rain.
		Higheft	Lowest	Mean.	In the House.			Abroad.			
					High.	Low.	Mean	High.	Low.	Mean	
Jan.	Morn.	29,77	28,32	29,15	42	31 $\frac{1}{2}$	35	43	20	29	3,308
	Aftern.				43	32	36	46	28	33 $\frac{1}{2}$	
Feb.	Morn.	30,05	28,49	29,25	46	33 $\frac{1}{2}$	40	45	22	34 $\frac{1}{2}$	1,946
	Aftern.				46 $\frac{1}{2}$	35	41	51 $\frac{1}{2}$	29 $\frac{1}{2}$	41	
Mar.	Morn.	29,81	28,56	29,30	48 $\frac{1}{2}$	38	43	44	28 $\frac{1}{2}$	36	2,728
	Aftern.				51	39	44 $\frac{1}{2}$	57 $\frac{1}{2}$	35 $\frac{1}{2}$	46	
Apr.	Morn.	29,77	28,72	29,24	53	44 $\frac{1}{2}$	48	52 $\frac{1}{2}$	32 $\frac{1}{2}$	42	1,523
	Aftern.				54 $\frac{1}{2}$	45 $\frac{1}{2}$	49	62 $\frac{1}{2}$	37 $\frac{1}{2}$	51	
May	Morn.	29,67	28,76	29,35	55	48	51 $\frac{1}{2}$	55 $\frac{1}{2}$	40	46	3,142
	Aftern.				56 $\frac{1}{2}$	49	53	69 $\frac{1}{2}$	45	57	
Jun	Morn.	29,76	28,87	29,33	62	54	59	61	50	55	2,485
	Aftern.				66	55 $\frac{1}{2}$	60	73 $\frac{1}{2}$	56	65 $\frac{1}{2}$	
July	Morn.	29,76	29,10	29,41	63 $\frac{1}{2}$	57 $\frac{1}{2}$	60	61	52	56	3,227
	Aftern.				66 $\frac{1}{2}$	58 $\frac{1}{2}$	62	76 $\frac{1}{2}$	61	66	
Aug.	Morn.	29,80	28,80	29,38	68	58	61 $\frac{1}{2}$	64	47	55 $\frac{1}{2}$	3,910
	Aftern.				70	60	63 $\frac{1}{2}$	78 $\frac{1}{2}$	59	67	
Sept.	Morn.	29,74	28,70	29,28	65	53	56	61	40	49 $\frac{1}{2}$	8,000
	Aftern.				68 $\frac{1}{2}$	53 $\frac{1}{2}$	57 $\frac{1}{2}$	73	48 $\frac{1}{2}$	59	
Oct.	Morn.	30,06	28,92	29,64	56 $\frac{1}{2}$	46	52	51	34	43 $\frac{1}{2}$	1,156
	Aftern.				57 $\frac{1}{2}$	46	53 $\frac{1}{2}$	64 $\frac{1}{2}$	42	53	
Nov.	Morn.	29,73	28,73	29,36	52 $\frac{1}{2}$	35 $\frac{1}{2}$	43	49	28	37	1,536
	Aftern.				52 $\frac{1}{2}$	36	44	55 $\frac{1}{2}$	32	41	
Dec.	Morn.	30,21	28,68	29,60	45 $\frac{1}{2}$	32	39 $\frac{1}{2}$	44 $\frac{1}{2}$	20	33 $\frac{1}{2}$	2,28.
	Aftern.				46	32 $\frac{1}{2}$	40	47	25	38	
35,235											

Wettest Months from one to twelve.		
1	Sept.	8,000
2	Aug.—Sept.	11,910
3	July—Sept.	15,137
4	June—Sept.	17,620
5	May—Sept.	20,762
6	April—Sept.	22,285
7	Mar.—Sept.	25,013
8	Feb.—Sept.	26,959
9	Jan.—Sept.	30,267
10	Dec.—Sept.	33,164
11	Nov.—Sept.	36,769
12	Oct.—Sept.	39,390

The quantity of rain this year having exceeded any I ever had before, I have added here a new table of the wettest months from one to twelve to those I had before given. See vol. LXI. p. 226. In the seventeen months from May 1773 to September 1774 there fell 55,890 inches of rain, which is 3,288 inches each month; and in the three years 1772, 1773, and 1774, there came 93,258 inches; that is, 31,046 inches each year. The year began very severe with a sharp frost, which was not out of the ground for seven weeks together, though in

the mean time there were several breaks, attended with great rains or snows and floods, and then freezing again. After the frost it was windy and wet for near a month, till above a week in March; the latter part of that month and all April were more fair, a good feed-time; and though with some frosty mornings, yet we had in general more mild weather than there has been in spring of late years. There was at times a good deal of fine weather in summer, yet mixed with a great deal of wet; it was a great grass year, and a cool summer. The hay-time and beginning of harvest were showery, yet more hindering than hurting; but the latter part of harvest in September was exceeding bad indeed. No grain could be carried for three weeks together; for it rained every day,

day, and in great quantities. I never measured so wet a month before. The wheat and oats were chiefly got in before it, and a great deal of barley; yet, as it was a late harvest, there was a great deal of the barley out, some wheat, and almost all the beans and pease. The wheat through the severe and wet winter was all along thin, and much of it mildewed by the wet towards harvest. The crop of barley was not amiss, if it could have been all well got; but some of it suffered by the wet after it was cut. The beans and pease were a remarkable great crop till harvest; but almost intirely spoiled in it. There was a great deal of winter meat for the cattle this year, plenty of good grafs, a great deal of hay, and fine crops of turneps; but the straw of that corn, which was out in the wet, was spoiled. The weather settled just at the beginning of October, which was a very fine month, almost like summer; and it was not till then that the harvest could be finished. The wheat seed-time was good, and the rest of the year favourable upon the whole; though a frost at the end of November and beginning of December was earlier than usual, attended with snow, and threatened a severe winter in most parts of Europe; yet it grew mild again afterward, was in general fair, and the ground continued tolerably dry, and a few frosty days concluded the year.



XIX. *An Account of some Thermometrical Observations, made by Sir Robert Barker, F. R. S. at Allahabad in the East Indies, in Lat.  $25^{\circ} 30'$  N. during the Year 1767, and also during a Voyage from Madras to England, in the Year 1774. Extracted from the original Journal by the Hon. Henry Cavendish, F. R. S.*

Redde, April 6, 1775. **T**HE greatest part of the observations at Allahabad were made within doors; several were made within a tent placed under the shade of trees, some in the open air in the Sun, and some in the open air in the shade; but there is no regular series of observations in any one place; nor were they made at stated times of the day. Though a thermometer kept within doors is but a very indifferent measure of the heat of any climate; yet as I have not seen any thermometrical observations made in that country, except a few during the heats of the summer, and printed in the Philosophical Transactions, vol. LVII. page 218. I have set down the great and least heights I meet with in each month.

	Least.	Greatest.		Least.	Greatest.
January	58	72	July	81	90
February	60	84	August	80	86
March	62	94	September	78	83
April	79	96	October	72	87
May	72	101	November	52	86
June	81	99	December	51	64

From the 3d of May to the 4th of June inclusive, a thermometer placed within a tent, under the shade of trees, was almost every day above  $100^{\circ}$ , and several times above  $109^{\circ}$ , once at  $112^{\circ}$ . The trees under which the tent was placed formed, as I have been informed, a very thick shade; so that I think these heights are more likely to fall short of the true heat of the open air at that time, than to exceed it. The least height I meet with of the thermometer in the open air in the shade, is  $42^{\circ}$ ; which it was at twice in the month of January, at 7 A.M. The greatest heat is on June 9th, at noon, when it was at  $114^{\circ}$ , the sky cloudy; the thermometer within doors at the same time  $95^{\circ}$ , which is less than it had frequently been in the month of May; so that it seems likely, that the heat in the open air in May had frequently been above  $114^{\circ}$ . During the voyage to England, the thermometer was placed in the round-house, and was observed regularly at eight in the morning, at noon, and at three in the afternoon; the winds and weather are also set down. The round-house, I have:

I have been informed, is one of the uppermost row of cabins, and is reckoned the coolest and most airy part of the ship. From February 13. to April 7. between Madras and the Southern tropic, the thermometer was constantly between  $77^{\circ}$  and  $86^{\circ}$ , and very seldom lower than  $80^{\circ}$ . From thence to April 23, lat.  $34^{\circ} 12'$ , about  $15^{\circ}$  E. of the Cape of Good Hope, between  $70^{\circ}$  and  $80^{\circ}$ . From thence to May 20, at St. Helena, between  $62^{\circ}$  and  $72^{\circ}$ . Thence to August 2. in lat.  $43^{\circ} 14'$  N. between  $71^{\circ}$  and  $80^{\circ}$ ; and from thence to August 15, in the British Channel, between  $62^{\circ}$  and  $70^{\circ}$ . At land it is well known, that the heat is usually considerably greater in the middle of the day than in the morning or night; but it appears from these observations, that in the open sea, there is scarce any sensible difference; for in settled weather, the difference between the different times of the day was rarely more than  $1^{\circ}$ , oftener none at all. In unsettled weather there was frequently a difference of  $2^{\circ}$ , sometimes  $4^{\circ}$ , scarce ever more; but then there seems no connexion between this difference and the time of the day, it being as often colder in the middle of the day than in the morning or evening, as warmer. There is added a register of the thermometer, in the soldiers barracks at Allahabad, on June 8, 1769, when from 10 in the morning to 8 in the afternoon it stood constantly above  $100^{\circ}$ , in the hottest part of the day at  $107^{\circ}$ , and during the whole night between  $99^{\circ}$  and  $98^{\circ}$ .

Sir Robert Barker, at my request, has been so good as to add the following account of the general state of the weather in Bengal.

THE rains at Bengal generally set in between the 1st and 15th of June, and continue till the middle of October, when it remains fair till February, the wind blowing mostly from the N.E. quarter, in which month and March it is interrupted by the N.W. squalls, attended with violent gusts of wind, thunder, and lightning, with short, but excessive hard, showers of rain or hail, commonly one, but rarely two, in each day. From the middle of March to the middle of June the weather is very hot. At Allahabad and the upper country the rains are not expected till the 20th of June, and seldom exceed the 30th, excepting in extraordinary seasons, when it has been known to keep off till the 5th of July; but such an event is usually attended with a great mortality both of men and beasts. They break up about the middle of September, and from this time to the beginning of January it continues fair cold weather. In January there are, almost always, a few days rain, seldom more than a week, and that gentle and pleasant, which is productive of a second crop, which they usually reap. The winds at Allahabad set in Easterly from the beginning of the rains, and blow almost constantly from that quarter until the conclusion of the cold weather in March, when it changes more Northerly, and is attended by violent North-west squalls of thunder, lightning, rain, and hail, at which time it changes to the West, blowing with violence, and a heat which frequently destroys the birds and beasts in the fields, till the rains afford a relief.

lief. The river Ganges begins to swell before the commencement of the rains, reported by the natives to proceed from the melting of the snow on the Northern mountains during the heats of May and June. But the sudden rise of the waters in the Ganges, a few days after the setting in of the rains, is almost incredible; since it has been known to rise twenty feet in forty-eight hours; and its sudden fall is as extraordinary. In Bengal the rivers are of course affected by the rise and fall of the Ganges. Floods continue the whole time of the rains, more or less; but the greatest overflowings are generally at the beginning and the end or the breaking up of the rains, at which period it rains with the greatest violence. The waters at Allahabad and in all the upper countries run off into the rivers as soon as the rain has ceased, the soil being for the most part of sand, and the country intersected with small rivulets; but in Bengal, and particularly so low down as Calcutta, being of a clay soil and an extensive flat, the whole country is overflowed, forming lakes of great extent, some of them being six miles over. The water, therefore, generally remains till the Sun has exhauled it, by which it becomes putrid, and renders those parts extremely unwholesome, occasioning those deadly putrid fevers, which carries off the patient in a few hours, known by the name of *pucker fevers*.

XVI. *A Second Essay on the Natural History of the Sea Anemonies. By the Abbé Dicquemare, Member of several Academies, and Professor of Natural Philosophy, &c. at Havre de Grace. Translated from the French.*

Redde, Mar. 9, 1775. **I** WAS concluding my essay on the sea anemonies inserted in the LXIII<sup>d</sup> volume of the Philosophical Transactions, when I discovered a fourth species of that animal, of which I could not at that time take a drawing; and I have reason to think, that I have since observed a fifth species. New obser-

*Second Mémoire pour servir à l'Histoire des Anémones de Mer. Par M. L'Abbé DICQUEMARE, de plusieurs Académies Royales des Sciences, Belles-Lettres et Arts, Professeur de Physique Expérimentale, &c. au Havre de Grace.*

Le Havre, 19 Août 1774.

**L**ORS que je terminois le mémoire inséré dans le LXIII<sup>me</sup> volume des Transactions Philosophiques, je découvris une 4<sup>me</sup> espece d'anémones de mer, dont je ne pus donner la figure. Je crois même en avoir depuis apperçu une

observations have encreased the number of my experiments: my ideas have been enlarged, my views extended; and the phænomena crowd in so fast upon me that I dare not flatter myself with the hopes of ever arriving at the end of this pursuit. The scarcity of high tides, the vicissitudes of seasons, and other similar impediments, make it less wonderful that a series of years should often elapse before it is possible to present the curious with any discoveries of which they might avail themselves, either by analysis, combination, or analogy, and thereby furnish general views and a chain of ideas leading to a new field of discovery, the usual effect of contemplation. I shall here endeavour as briefly as possible, to communicate some of the ideas that have been suggested to me by my last experiments.

How many are the animal functions, which seem to depend upon sensibility and irritability; and yet how little are these faculties understood? how ignorant are we of their

---

5<sup>ème</sup>. De nouvelles observations ont multiplié mes expériences, mes vues se sont étendues, mes desseins se sont accumulés, et les phénomènes se succèdent de manière à me faire croire que j'en verrai difficilement la fin. La rareté des grandes marées, la vicissitude des saisons amène des années, des suites d'années, avant de pouvoir offrir aux savans quelques découvertes dont on puisse tirer avantage par l'analyse, la combinaison, l'analogie; et ouvrir par des vues générales, par la liaison des idées, &c. de nouvelles routes, fruits ordinaires de la contemplation. Essayons sans prolixité d'exposer ici quelques unes des vues que m'ont fait naître mes dernières expériences.

De combien de fonctions ces deux facultés trop peu connues, la sensibilité et l'irritabilité, ne paroissent-elles pas être la cause? mais combien encore la leur nous est-elle cachée? Quoique les nerfs semblent être chez nous les principaux,

their cause? The nerves seem to be the chief, perhaps the only, organs of sensibility in man, and the muscular fibres to be the principal seat of irritability; yet how many are the doubts entertained concerning the parts that are and are not endowed with one and the other! how false and erroneous the conclusions relating to the effects they produce, notwithstanding the many experiments made on animals, whose interior structure is the most similar to our own! It is then from accurate observations on such animals as bear the least resemblance to our species that we may hope for new discoveries. The sea anemonies are exceedingly gelatinous, and at the same time so irritable, that even light affects them, though to all appearance destitute of eyes. Might not the rapid and singular reproduction of the parts of this animal be attributed to their gelatinous texture? and if so, may we not reasonably conclude, that the reproduction of our vascular and fleshy parts in the consolidation of wounds is in  
great

---

cipaux, peut-être même les seuls organes de la sensibilité, et les fibres musculaires le siège de l'irritabilité; que de doutes sur les parties qui en sont, ou n'en sont pas douées! que de fausses conséquences sur leurs effets, malgré les expériences qu'on a faites sur les animaux qui se rapprochent le plus de l'homme par leur structure intérieure! C'est donc en examinant avec une attention scrupuleuse ceux qui s'en éloignent le plus que nous pouvons espérer de nouvelles découvertes. Les anémones de mer, animaux extrêmement gélatineux, sont si irritables, que la lumière les affecte, quoiqu'ils ne paroissent point avoir des yeux. Une substance aussi gélatineuse ne seroit-elle point la cause principale de la prompte et singulière reproduction de leurs parties? et si cela est, ne pourrions nous pas penser que dans la reproduction de nos chairs et de nos vaisseaux, dans la consolidation des playes, &c.



great measure owing to such a gelatinous matter; and should we not seek for means to increase or diminish the quantity of that matter as circumstances may require?—I cannot quit this subject without reluctance.—If it be true, that earth and a gelatinous substance be the constituent parts of the muscular fibres of such animals as we are best acquainted with, and that only the latter be capable of irritation; doth it not follow, that the gelatinous nature of the sea-anemonies is the true cause of the effect produced upon them by the impression of light? and may we not conjecture, from the very gelatinous nature of these animals, and from their being affected by light on every part of their bodies, but more particularly on those that are recently cut; may we not, I say, from hence conjecture, that the gelatinous part of the muscular fibre is the only one capable of irritation in ourselves? Might not these animals, by a sober use of analogy, or by new experiments, lead us to a more

---

la partie gélatineuse joue un grand rôle, et chercher les moyens de l'augmenter ou de la diminuer selon les circonstances. J'ai peine à abandonner cet objet. S'il est vrai que dans les animaux qui nous sont plus connus, les fibres musculaires sont composées d'une partie terreuse et d'une partie gélatineuse, et que cette dernière soit seule irritable, l'effet que produit la lumière sur les anémones de mer n'aurait-il point pour cause leur substance gélatineuse? ou ne pourrions nous pas conjecturer (de ce que ces animaux sont très gélatineux et affectés de la lumière par toutes les parties de leur corps, plus même par celles qui sont récemment coupées) que c'est réellement chez nous la partie gélatineuse de nos fibres musculaires qui est la seule irritable? Ces animaux ne pourroient-ils point, par une sorte d'analogie, dont pourtant il ne faut pas abuser, ou par de nouvelles expériences, nous faire mieux

more perfect knowledge of those singular enemies to man, the tape, the hair worm, and the sea dragon? I shall, however, leave these researches, to which nature seems to invite us, and from which useful discoveries may probably be derived, to those who apply solely to physiology; and shall here content myself with laying before the public, the observations and experiments to which I have confined my researches..

I continued to observe the inferior half of a purple; anemony of the first species, which I had cut in two on the 12th of July 1772, and which was alive on the 8th of April 1773, the day on which I concluded my former essay: it appeared to me to be daily recovering strength. On the 26th I found it at the bottom of the vessel. On the 1st of July it climbed up the sides almost to the surface of the water; and this it repeated on the 15th and 22d, above a twelvemonth after the time it had been cut. On the 25th a crab (*cancer lanosus*, *cancer venenatus*) half dried,

---

mieux connoître ces singuliers ennemis de les crinons, l'homme les draconcules, le ténia et autres? c'est à ceux qui s'occupent uniquement de la physiologie à suivre ce sentier, que la nature nous présente, et qui pourroit conduire à un champ fertile. Restreint à mes petites observations, et à mes expériences, je me contenterai de les faire passer sous les yeux du public.

La moitié inférieure du corps d'une anémone pourpre de la première espèce, que j'avois coupée le 12 Juillet 1772, et qui avoit vécu jusqu'au 8 Avril 1773, jour où je terminois mon premier mémoire, continua d'être observée; elle se fortifioit de jour en jour. Le 26 je la trouvai au fond du vase, le premier Juillet elle monta le long des parois presque à fleur d'eau, le 15 et le 22, c'est-à-dire plus d'un an après qu'elle eut été coupée, elle y monta encore. Le 25, un cancer (*cancer lanosus*, *cancer venenatus*) à demi desséché tomba dans le vase, y séjourna quelques

dried, fell into the vessel, and after continuing in it some hours, infected and tinged the water in the same manner as if husks of walnut or pieces of foot had been thrown into it, which had such an effect upon the piece of anemony, that it threw up a great quantity of its intestines. On the 30th it laid hold of the side again, but was considerably shrunk. In the beginning of September it received a second injury, from another piece of anemony, which having been damaged in the same manner by the former accident, suddenly putrified and infected the water: more of the intestines were now discharged; and this last accident, added to the former one, affected the creature to such a degree, that it wasted gradually till the 8th of October, when it was totally dissolved. The sea-anemonies are undoubtedly susceptible of irritation to a very great degree; but is all that hath been described to be considered as the mere effect of irritability? Allowing that to be the case, will it not follow, that we are more in the dark concerning that faculty than  
is

---

quelques heures, empoisonna et teignit l'eau, comme l'auroit fait le brou de noix ou un morceau de suie: la partie d'anémone en souffrit, jusqu'à jeter dehors une quantité d'intestins. Le 30 elle s'attacha encore, elle étoit alors diminuée de volume. Ayant eu à souffrir dans les premiers jours de Septembre d'une autre partie d'anémone, qui avoit essuyé le même accident, et qui se corrompant tout à coup gâta l'eau, quelques intestins lui sortirent de nouveau; ce dernier accident suite du premier l'affecta si fort, qu'elle diminua de plus en plus jusqu'au 8 Octobre, qu'elle étoit totalement décomposée. Les anémones de mer sont sans doute très irritables; mais peut-on rapporter tout ce qui vient d'être exposé au seul effet de l'irritabilité? et si cela étoit, cette faculté ne nous seroit-elle pas encore moins connue

is generally thought? It is usual to ascribe to it the palpitation that is perceived in the flesh of oxen, when cut from the animal, in the severed pieces and hearts of some reptiles, as the floth, and other involuntary spasmodic motions; but is it possible, that determinate motions, that actions which seem to imply will, such as clinging, &c. which in our experiment were continued for the space of fifteen months, and, but for an accident, might probably have been carried on much longer, should arise from meer irritability without any other cause? The upper part of another sea-anemony, of which the inferior was become a perfect animal, lived six months after its being cut, and seemed to feed by suction upon pieces of muscle I put in its way. Sea-anemonies; which I had cut diametrically and perpendicularly, were not essentially hurt by that operation; which might be expected to disorder more than any the whole animal oeconomy, and to be particularly injurious to the basis of this animal, which

---

connue que nous ne le pensons? On y rapporte communément les mouvemens qu'on apperçoit dans la chair de boeuf séparée de l'animal, aux tronçons, au coeur de certains reptiles, à celui des paresseux, &c. mais des mouvemens déterminés, des actions, comme celle de s'attacher, et autres qui dans cette expérience ont duré quinze mois, et qui auroient été plus loin, peuvent-ils n'avoir que l'irritabilité pour cause? La partie supérieure d'une autre anémone, dont l'inférieure étoit devenue un animal entier, a vécu six mois, et paroissoit se nourrir en suçant des morceaux de moule que je lui présentois. Des anémones de mer coupées diamétralement et perpendiculairement ont soutenu fort bien cette opération, qui semble devoir déranger plus que toute autre l'oeconomie animale, et offenser considérablement la base, qui est dans ces animaux une partie très essentielle, et dans quelques

which is its most essential part, and in some species is exceedingly tender. The two sides soon came together, but but were some time in contact before they connected. The junction, however, was at last so perfect, that no visible scar remained upon the robe, the continuity of the little blue edge was not in the least interrupted, and the mouth was perfectly restored. These semi-anemonies, which I still preserve, have long since acquired the appearance of the perfect animal, and perform all its functions, such as moving from place to place, swallowing, &c. This leads to the reflection that if, as hath been asserted, the power of loco-motion in these animals depends upon a certain combination of straight and circular tubes, it is not requisite, in order to exert it, that the continuity of these tubes be uninterrupted, since half an anemony newly cut changes its place with as much ease as a whole one. It will no doubt appear a curious enquiry, whether these semi-anemonies, after becoming in a manner whole ones, are capable of propagating their species.

---

quelques especes, très délicate. Les deux cotés se sont rapprochés; mais ils ont été long temps à se joindre solidement; cependant cette jonction fut opérée au point qu'il ne restoit aucune marque sur la robe. La petite bordure bleue n'étoit nullement interrompue, et la bouche s'est reformée. Ces moitiés d'anémones que je conserve, et qui ont depuis longtemps tout l'air d'un animal entier, en font toutes les fonctions, comme de changer de lieu, d'avaler, &c. Sur quoi il faut remarquer que s'il est vrai, comme on la cru, que ces animaux changent de lieu au moyen de canaux droits et de canaux circulaires, du moins il est vrai aussi qu'il n'est pas nécessaire que ces canaux soient entiers, puisque des moitiés d'anémones nouvellement coupées ne laissent pas de changer de place aussi facilement que l'animal entier. On seroit sans doute curieux de savoir si ces moitiés d'anémones, devenues

species. To this I can only answer at present, that I have not yet seen the generation of anemonies, except in the sea, or in animals newly taken out of it. It must further be observed, that these anemonies, perfect as they seem to be, may perhaps have only half the number of limbs of the whole ones, whereof they made a part: so that the whole wonder comes to this; that the severed halves of an animal should recover, and each taking the appearance of an entire individual, continue to live as if they were such. And such in fact I believe they are; but this I have not yet been able to ascertain, as the anemonies of this species have not all the same number of limbs, as it is always very difficult to count them, and as all those upon which I have hitherto made the experiment had a great number of them. However, as no manner of difference appears, I am inclined to suspect that new limbs shoot out between the old ones.

After

---

devenues en quelque sorte des animaux entiers, donnent des petits; il suffit de répondre ici que je n'ai jamais vu naître des petits que des anémones qui étoient en mer, ou de celles qui en étoient tirées depuis peu de temps. Il faut encore observer, que ces prétendus animaux complets n'ont peut-être que la moitié des membres qu'avoit l'animal dont ils faisoient partie, et qu'ainsi le merveilleux de cette opération se réduiroit à voir chaque moitié d'un animal se guérir, prendre la forme de l'animal entier, et vivre comme s'il l'étoit. Je crois qu'ils le sont en effet; mais je n'ai pu encore le vérifier exactement, parceque les anémones de cette espece n'ont pas toutes le même nombre de membres, qu'il est très difficile de les compter, et que toutes celles que j'ai opérées en avoient beaucoup. L'apparence est absolument la même, peut-être leur pousse-t'il des membres entre les autres.

After having observed these animals during several years, both in the sea and in my study, it will no doubt be expected, that I should now give a particular account of their manner of propagation; but here I can only confess my ignorance, having never been able to get at the knowledge of any one circumstance relating to it: which makes me suspect, that these animals propagate without any communication of individuals. What I would here suppose is by no means unexampled. Among the *aphides*, for instance, (whose mode of propagation deserves to be further examined) though the sexual parts have been discovered, individuals nevertheless are found, which, though deprived of all communication one with another from the very moment they are brought forth, yet produce an offspring, which being likewise denied all intercourse, still propagates; and so on, through a great number of generations, which succeed each other very rapidly. The muscle also is thought to be an animal of the same nature.

The

---

On s'attend peut-être qu'après avoir observé ces animaux pendant plusieurs années à la mer et dans le cabinet, je vais exposer ici tout ce qui concerne leur génération : point du tout ! il ne m'a pas été possible d'en découvrir la moindre chose ; ce qui m'a porté à croire, que cette espèce se multiplie sans qu'un individu ait besoin de la participation d'un autre. Ce que j'établis n'est pas sans exemple. Les pucerons, dont la génération mérite encore d'être examinée, quoiqu'on y ait découvert des parties sexuelles, offrent, à ce qu'on assure, des individus qui séparés les uns des autres dès la sortie de la mère, donnent des petits, et ceux-ci privés de même de toute communication en donnent encore, ce qui a été poussé jusqu'à un grand nombre de générations, parcequ'elles se succèdent promptement : mais pour ne pas sortir des eaux ; la moule paroît dans le cas que nous supposons.

La

The anemonies of the second species are not only less obvious to our observation, but it is with difficulty they are preserved in any degree of perfection. They cannot be taken out of the sand without depriving them of their natural position. Common mixed sand kills them in a few days; and that which is purified affords them no longer the slime, the small insects, or other necessary sustenance, which we cannot possibly divine. In plucking them from their native soil their bases generally suffer, and the wounds in that part are frequently mortal. One of the safest expedients is to gather with them the pebbles to which they adhere; or what is still preferable, to observe them in their natural element the sea. It is there that, without the least hostile appearance, they are seen to make an incredible havoc. I have seen an anemony of a moderate size swallow a smelt at least six inches long. The limbs of this species, which are much thicker than those of any other, being clipped, new ones shoot out as in former cases. The progress of this reproduction,

---

La seconde espèce est non seulement plus difficile à observer que la première, mais il n'est pas facile de la conserver en bon état. Hors du sable elle n'est plus dans sa position naturelle. Celui qui n'est pas purifié la fait périr en peu de jours; celui qui est net ne lui fournit plus le limon, ou les petits insectes, ou quelque autre commodité, que nous ne devinons pas. On arrache la base presque toujours, plus ou moins, en la détachant, et ces plaies sont souvent mortelles. Le moyen le plus certain c'est d'apporter avec elle, les cailloux sur lesquels elle est attachée, ou, ce qui est bien mieux, de l'observer à la mer. C'est là que sous l'extérieur le plus paisible, on lui verra faire un dégât étonnant. J'ai vu telle anémone de moyenne grosseur avaler un éperlan de six pouces de long. Les membres de cette espèce beaucoup plus grosse que les autres repoussent aussi après avoir été coupés. Les pro-



duction, which is effected in a few days, is scarce perceptible; and it is so perfect, that no protuberant rim or visible scar remains. Neither the colour, the size, nor the form, are any ways altered. This anemony is able to creep when deprived of its limbs; which seems to prove, that the communication, which is thought to exist between the limbs and the hollow muscles, may be interrupted, without sensibly restraining the animal's locomotive powers. Those limbs, it is true, enable the animal to crawl when turned on its back; but do by no means serve as legs for walking steadily, as hath been erroneously asserted, and misrepresented in ill-drawn figures. I made large incisions on several anemonies in the sea, which healed in a very short time; but I always took care not to injure the basis, as any considerable wound, and especially the least rent, on that part of this species proves often mortal. I do not mean to question the possibility of what hath been repeatedly said of an

---

grès de cette reproduction, qui s'opère en peu de jours, sont difficiles à saisir et à suivre, elle est si parfaite qu'il n'y a aucun bourrelet. La couleur, la grosseur, et la forme sont les mêmes. Lorsque l'anémone a les membres coupés, elle ne laisse pas de marcher: ce qui prouve que si, comme on la cru, il y a communication entre les membres et les muscles creux, elle peut être interceptée sans que les mouvemens en soient sensiblement gênés. Ces membres servent bien à l'anémone à se trainer, lorsqu'elle est retournée ou renversée; mais ils ne lui servent pas de jambes pour marcher ferme, comme on l'a prétendu, et comme le représentent des figures mal dessinées. J'ai fait de grandes incisions à des anémones qui étoient en mer; elles se sont cicatrisées; mais j'épargnois la base: car les blessures considérables y sont souvent mortelles dans cette espèce, sur tout les déchiremens. Ce qu'on a dit et répété d'une anémone, qui ne pouvant rendre une moule, l'avoit fait

an anemony, which not being able to void a muscle it had swallowed, forced it out through a rent it made with the muscle itself at its basis, and that this rent was soon after perfectly cicatrized. But a love of the marvellous too plainly appears throughout the whole narration, and the inferences drawn from the fact give room to suspect, that little attention had been paid to the concomitant circumstances. Wounds of this nature often occasion a disorder in the interior part of the anemony, the progress of which soon brings on its total dissolution. Of all the kinds of sea-anemonies I should prefer this for the table: being boiled some time in sea-water they acquire a firm and palatable consistence, and may then be eaten with any kind of sauce. They are of an inviting appearance, of a light shivering texture, and of a soft white and reddish hue. Their smell is not unlike that of a warm crab or lobster. I have seen some of the young of this species, but have been able to make no discovery concerning their mode of propagation. A de-

---

fait sortir en faisant avec cette moule une déchirure à sa base, laquelle s'étoit cicatrisée peu après, n'est pas impossible: mais l'amour du merveilleux s'aperçoit dans la manière de rapporter le fait, et les conséquences qu'on en tire font voir qu'on avoit peu observé avant et après. Ces sortes de playes laissent souvent dans l'intérieur une maladie dont le progrès fait périr l'anémone. De toutes les espèces d'anémones de mer, celle-ci m'a paru devoir mériter la préférence pour la table. Lorsqu'on les a fait bouillir un peu longtemps dans l'eau de mer, afin qu'elles soient bien cuites et un peu fermes, et qu'on les sert pour manger à quelle sauce on juge à propos, elles ont un air ragoutant, une consistance légère et tremblante, une couleur douce mêlée de blanc et d'un rouge agréable, et une odeur d'écrevisse ou autres cancrs encore chauds. Je n'ai rien pu découvrir sur la génération de cette espèce: j'ai vu seulement de jeunes individus.

A detail of my former observations and experiments would be here an useless repetition: I shall therefore only observe, that the semi-anemonies of the third species have so entirely recovered the parts they had been deprived of, whether the superior or the basis, that no manner of difference could be perceived. Some of the men of learning, whom my first discoveries brought to my study, imagined that the basis was the most essential part of the animal, and that the mouth and limbs were to be considered only as extremities. I was myself inclined to adopt that notion, seeing that in all the species abovementioned, the basis ever gave the greatest marks of sensibility, that the intestines are situated in that region, &c. but who, on seeing the upper part of an anemony producing a new basis perfectly similar to that which had been severed from it, will any longer maintain such an opinion? During the great equinoxial tides, in places from whence the sea seldom

---

Le détail de mes observations et de mes premières expériences seroit ici une répétition inutile : il suffira de dire, que les moitiés d'anémones de la 3<sup>ème</sup> espece ont repoussé si parfaitement ou la base ou la partie supérieure qui leur manquoit, qu'il n'est pas possible de s'en appercevoir. Parmi les savans que mes premières découvertes attiroient à mon cabinet, il s'en trouva qui crurent que ce qui constitue essentiellement l'animal étoit dans la base, et que les membres et la bouche devoient en être regardés comme les extrémités. J'étois moi même tenté de le croire, d'autant que la base m'a toujours paru plus sensible dans toutes les especes, et que c'est dans cette région que sont contenus les intestins, &c. mais quelle apparence de rester dans cette persuasion, lorsqu'on voit la partie supérieure reformer une base tout à fait semblable à celle qui a été retranchée? Pendant les grandes mers d'équinoxe,

feldom recedes, I saw several of these animals which had been cut through the middle, perhaps by some crab, by the sudden collision of pebbles, or by some other means, which though not unnatural, we may yet not be able to account for. They soon began to recover. I should have taken them for a new species, had not my former experiments pointed out to me the gradual reproduction with which nature, no less various than impenetrable in her resources, kindly indulges them. Are not the accidents which happen to birds, quadrupeds, and even to man, frequently followed by effects, which seem intended to convince us, that we lay too great a stress on the resources of art, and trust less than we ought to do to nature? Although I could never yet arrive at any certain knowledge concerning the generation of this species, I suspect that it is different from that of the others. Several of my specimens have suddenly let fall to the bottom  
of

---

aux lieux d'où la mer se retire rarement, j'ai aperçu plusieurs de ces animaux qui avoient été coupés par la moitié du corps, peut-être par quelque cancre, par le choc subit de quelque caillou, ou par quelque autre moyen même naturel que nous ne pouvons pas prévoir; ils commençoient à se rétablir. Je les aurois pris pour une espèce différente, si les expériences que j'avois tentées auparavant ne m'avoient fait connoître le rétablissement gradué que leur accorde la nature, dont les ressources sont d'autant plus inconnues qu'elles sont sans nombre. Ne voit-on pas de temps en temps, à l'occasion des accidens qui arrivent aux oiseaux, aux quadrupèdes, et à l'homme même, des effets qui semblent destinés à nous convaincre, que nous comptons trop sur les ressources de l'art, et trop peu sur celles de la nature. Quoique je n'aye eu aucun éclaircissement sur la génération de cette espèce, je soupçonne qu'elle est différente des autres. Plusieurs individus ont jetté  
tout

of the vessel in which they were kept, a slimy substance, nearly of the colour of their bodies, perhaps somewhat yellower, which, in the microscope, appeared to consist of a great number of globular particles, pretty much resembling the spawn of fish.

The first anemonies I procured of the fourth species, had probably been brought near the coast by fishermen, for they generally keep in deep water where they are found adhering to oyster-shells. I caused several to be brought into my study, where being put into sea-water they soon expanded. The largest, which opened first, puzzled me not a little. I could discover no basis, but only saw limbs projecting on every side. I flattered myself that a greater expansion would clear up the difficulty; on the contrary it only added to it. The others opened, and appeared in a shape much more similar to that which I expected. I saw a basis, a body, a great

tout à coup au fond du vase où je les conservois une espece de limon, de la couleur de leur corps, ou un peu plus jaunâtre, lequel examiné au microscope paroît formé d'une très grande quantité de globules qui auroient assez l'air d'un fray.

Les premières anémones de la quatrième espece que je trouvai avoient vraisemblablement été apportées au bord de la mer par les pêcheurs: car elles se tiennent au large, et sont attachées sur les huîtres. J'en fis apporter plusieurs à mon cabinet et les ayant mises dans l'eau de mer, elles s'ouvrirent. La plus grosse, qui s'ouvrit la première, me donna bien de la tablature. Je ne savois où en pouvoit être la base, appercevant des membres de tous cotés; j'attendois à être instruit par un plus grand épanouissement: mais plus il avançoit moins je voyois clair. Les autres individus s'ouvrirent, et se présentèrent sous une forme plus approchante de celle que j'attendois. Je vis une base, un corps, une grande quantité de membres très

great number of slender limbs, the assemblage of which formed, at first, different kinds of tufts, and afterwards various fine plumes of a whitish hue inclining to carnation. I returned to my first specimen, which now appeared to consist of two animals joined at the basis. I became very solicitous to unravel the mystery of this singular union. At length I perceived, that this was a monster of its kind, consisting of three different animals blended into one. It perished twelve days after I had received it. Its internal structure, though in great confusion, was yet an interesting object to those who are acquainted with that part of this animal, and who have a taste for comparative anatomy. It appeared in such disorder that I can scarcely conceive how it was possible for the creature to live in that condition. The state of dissolution, which began soon after, and the impossibility of representing every part at one view, prevented my taking a drawing of this remarkable inside. Its mouths

---

très déliés dont l'assemblage formoit des especes de houpes, ensuite de beaux panaches, le tout blanc tirant un peu sur la couleur de chair. Je retournai à mon premier individu, qui me parut double, les bases étoient réunies. Quelle est cette union singuliere, me disois-je a moi même. Enfin je m'aperçus que c'étoit une anémone monstrueuse, dans laquelle trois individus sembloient être confondus. Douze jours après, cette anémone a péri. Sa structure intérieure, quoique très confuse, ne laissoit pas d'être intéressante à voir, pour quiconque connoit celle de ces animaux, et a du goût pour l'anatomie comparée. Elle étoit dans un bouleversement si grand que j'ai peine à comprendre, comment l'animal avoit vécu. Il ne m'a pas été possible de dessiner cet intérieur à cause de l'état de décomposition qui commençoit, d'ailleurs il falloit voir partie après partie, ce qu'il n'étoit pas aisé de bien rendre. Ses bouches de coté et d'autre étoient bien

mouths on either side were regularly shaped, but rather less than the usual size; and in the folds, formed by the bases, several limbs appeared, which seemed to belong to a third animal, incorporated in the two that were more apparent. The sequel will shew, that this is not the only peculiarity I have observed in this species, which by its manner of propagating seems particularly calculated for producing monsters. The anemonies of this kind are commonly found adhering to the convex shells of oysters: they abound in the road of Havre de Grace, so that I had no difficulty in procuring whatever number I chose. A viscose matter, like that which is seen on fish newly caught, issues from them. I have opened between two or three hundred of a large size, but I never found in any of them either whole or parts of animals; and yet as often as I offered them a piece of oyster or muscle, they would swallow it. The large anemonies of this species are generally surrounded by a multitude of small and middle-sized ones,

---

faites, seulement un peu petites. Dans les duplicatures que formoient les bases on appercevoit des membres qui paroissent appartenir à un troisieme individu confondu dans les deux plus apparens. On verra par la suite que ce n'est pas la seule singularité que m'aït offerte cette espece, qui paroît très propre, par sa maniere de se perpétuer, à donner des productions monstrueuses. C'est ordinairement sur l'écaille la plus convexe des huitres que ces anémones se trouvent attachées: il y en a dans les rades du Havre une quantité prodigieuse; il ne m'a donc pas été difficile de m'en procurer. Elles rendent une matiere visqueuse comme celle du poisson nouvellement pêché. Quoique j'en aye ouvert de deux à trois cens fort grosses, je n'ai jamais trouvé dedans aucun animal ou portion d'animal: mais lorsque je leur ai présentée des morceaux d'huitres ou de moules, elles les ont avalés. Autour d'une grosse anémone de cette espèce on en voit ordinairement

ones, which form very pleasing groups (see the figure, TAB. VI.). In searching into the mystery of this numerous progeny, I observed that the bases of some of these small anemonies were not perfectly round, and that in others they mutually adhered one to another: nor was this communication nothing more than apparent; for whenever I slightly touched with a pointed instrument, the basis between two connected anemonies, they both contracted at the same time. I also observed, that this common basis distended itself gradually near the middle, where it assumed the appearance of a net, which bursting at length, left every small anemony to live by itself. I moreover perceived, that there issues out of the body of the anemonies of this species, through little pores, and also out of their mouths, a considerable number of round, soft, limber threads, of the thickness of a horse-hair, and of the colour of the animal. On viewing them through a lens, I observed a  
great

---

une multitude de petites et de moyennes qui forment de très jolis groupes (voyez la planche). En recherchant tout ce qui pouvoit me dévoiler le mystère de cette nombreuse progéniture, je remarquai, qu'il y avoit certaines petites anémones dont la base n'étoit point ronde, et d'autres unies entr'elles par la base. La communication n'étoit pas seulement apparente: car lorsque je touchois légèrement avec une pointe fine la base entre les deux petites anémones, elles se fermoient en même temps; je remarquai de plus, que cette base commune s'élargissoit peu à peu par le milieu, et y devenoit comme un filet, et qu'enfin elle s'y rompoit, ce qui procure à chaque petite anémone la faculté de vivre en particulier. J'observai encore qu'il sort du corps des anémones de cette espèce, par de petits trous, et de leur bouche, un nombre considérable de cordons ronds, mous, déliés, de la grosseur d'un crin, et de la couleur de l'animal: je les examinai à la loupe; ils me parurent



great resemblance between them and the spermatic vessels of men, when stripped of their outward sheath. Through a common microscope I saw fibres in them which crossed each other in every direction : and by means of a solar microscope, which magnified them to a diameter of five inches, they appeared of a very close texture, which, when decomposed, seemed to consist of an infinite number of vessels, crossing each other in almost every direction ; but farthest extended lengthwise. A liquor seemed to circulate in the largest of these vessels, which, where they meet, form kinds of ganglions like the optic nerves in man. Such an organization cannot surely but be intended for valuable purposes. Is it not probable, that these threads contain certain knots, bulbs, knobs, or buds, which open in time, and cleaving to the bodies on which those threads are extended, produce small anemonies, which at first communicate one with another, but afterwards separate by a contraction,

---

à peu près figurés comme les vaisseaux spermatiques dans l'homme dépouillés de leur gaine. J'y voyois au microscope simple des fibres croisées en tout sens, et au microscope solaire apperçues sous un diametre de cinq pouces, on voyoit un tissu serré, qui venant à se décomposer laissoit appercevoir une quantité immense de vaisseaux croisés presque en tout sens, et qui cependant s'étendoient plus sur la longueur. Une liqueur paroît circuler dans les plus gros, qui en se rencontrant forment des especes de ganglions comme les nerfs optiques dans l'homme. Une telle organisation est sans doute destinée à des usages précieux. N'y auroit-il point dans ces cordons certains nœuds, bulbes, boutons, œuilletons, qui venant à s'ouvrir et à s'attacher sur les corps où ils sont étendus, y formeroient autant de petites anémones qui d'abord communiqueroient l'une à l'autre, et se sépareroient.

traction, as I have myself observed in some of them. This I concluded from my having never found any young ones in the great number of anemonies I have opened; at the same time that I have seen prodigious quantities, of a very small size, adhering to oyster shells. This, however, amounted to no more than a conjecture; which not being confirmed by subsequent observations, I might have been tempted to conclude from analogy, that (as in the case of the first species) the young ones are brought forth, ready formed, through the mouth, at certain seasons in which I have not yet opened any of these anemonies. Analogy is too frequently misapplied, and in this instance it would have led me into error. A knowledge of the operations of nature is to be acquired by close inspection, not by loose conjectures. From a series of observations, which to avoid prolixity I shall not here repeat, I have learnt, among other singularities, that these animals having their bases irregularly distended, and their extremities closely adhering to some hard body,

commonly

---

ensuite par étranglement, comme j'en ai vu. Je raisonnois ainsi, n'ayant trouvé aucuns petits dans les anémones que j'avois ouvertes, tandis que j'en voyois prodigieusement et de très petites sur les huîtres. Ce n'étoit donc qu'une conjecture qui, ne se réalisant pas, auroit pu me laisser penser par analogie que, comme dans la première espèce, les petits naissoient tout formés par la bouche, dans certaines saisons où je n'avois pas ouvert de ces anémones. Cette analogie, dont on abuse si souvent, m'auroit trompé. Il faut voir, et non pas deviner les opérations de la nature. Des suites d'observations, que je ne puis rapporter ici sans prolixité, m'ont appris entr'autres singularités, que ces animaux ayant la base inégalement étendue et fortement attachée par les extrémités sur un corps dur, souvent

une

commonly an oyster shell; they, by suddenly shrinking, leave upon that body some small portions of the rim of their bases, in size inferior to a lentil. These little shreds have, at first, no determined figure; but gradually assume the rounded shape of a drop of tallow: at length, in the space of two or three months a hole appears in the middle, which forms the mouth. An internal organization, dilatations and contractions, sensibility, and other gradual improvements, soon after prove them to be animals similar to those to which they owe their origin. It might be imagined, that some time must elapse before they can grow to a circumference of two feet. I have not been able to follow them to that degree of increase; but I have seen them in my house, where they are far from being so favourably situated as in the sea, growing to a size large enough to convince persons of ever so little observation, that they belong to the species of the large anemonies. The same shred often produces  
several

---

une grosse huitre, ils se retirent sur eux mêmes, et laissent par ce moyen sur ce corps une ou plusieurs petites portions du bord de leur base, moins grosses qu'une lentille. Ces petits lambeaux paroissent d'abord informes, ils s'arrondissent peu à peu en goutte de suif; enfin dans l'espace de deux à trois mois, on y observe un trou dans le milieu, c'est la bouche; une organisation intérieure, des dilatations, des contractions, la sensibilité, et autres progrès successifs, qui en font une anémone semblable à celle dont ils tirent leur origine. Il paroît qu'avant d'avoir acquis deux pieds de circonférence, cela doit être long. Je n'ai pu les suivre jusqu'à ce terme: mais je les ai vues chez moi où elles ne sont pas dans des circonstances semblables à celles de la mer, devenir assez grandes pour que les gens les plus grossiers pussent les reconnoître pour appartenir aux grandes anémones. Sou-

several small anemonies, which at first adhere together, and in time are separated by the little contraction I have already mentioned; but if they happen to remain connected, they then produce singular forms and often monsters. Besides the anemony abovementioned, the old one of this species, which hath particularly unravelled this mystery to me, was formed in the shape of a Y, having two perfect bodies, whereof the bases, both perforated, adhered and communicated to the same trunk; of which I convinced myself by observing the food which descended into the main trunk: neither did these two anemonies, thus connected, ever appear to me to have different inclinations, as is the case with those that are once separated. Is it not reasonable therefore to suppose, that in this state of union, every want was common, and each had its separate desire of satisfying it; and that, to keep up the habitual exercise of the animal functions in each, both were upon all occasions prompted to the performance.

---

vent plusieurs petites anémones se dévelopent du même lambeau, de sorte qu'elles sont adhérentes entr' elles. Peu à peu il s'y forme, comme je l'ai fait connoître, un petit étranglement qui les sépare. Quelquefois aussi elles restent unies: alors il en résulte des singularités, ou même des monstres. . . Outre celui que j'ai cité, l'anémone mere de cette espece, qui m'a dévoilé plus particulièrement le secret, étoit formée comme un Y, c'est à dire qu'elle avoit deux corps parfaits dont les bases (cependant percées) étoient adhérentes à une même tige, à laquelle ils communiquoient, ce dont je me suis assuré, en observant la nourriture qui descendoit jusques dans le gros tronc: aussi ces deux anémones ainsi réunies ne m'ont-elles jamais paru avoir deux volontés comme celles qui ne le sont pas. Chaque anémone n'avoit-elle donc point dans celle-ci d'inclination particuliere à satisfaire au besoin commun? n'étoit-elle point sollicitée à faire ses fonctions pour en entre-

tenir

formance of the same function at the same time? It is to be presumed, that two similar united quadrupeds would vie with each other in taking the necessary nourishment, in order thereby to satisfy the particular need, which each would have, of keeping up the habit of deglutition, and the dependant functions. (Vide TAB. VI.)

In order to imitate the effects of nature, I clipped several small pieces from the rim of the bases of anemonies of this species, and preserved them. Some of them became small anemonies, similar to those that had been torn off of their own accord; but many perished without producing any thing. May we not conclude from this experiment, that the prolific pieces contained a small bulb intended to become a new anemony, and to be soon after torn off by the mother; and that those which perished, either contained none of those bulbs, or such as were not sufficiently formed to thrive and grow after a violent

---

tenir l'usage? Il semble que deux quadrupedes réunis et parfaitement égaux, chercheroient tous deux à manger, peut-être à l'envi l'un de l'autre, pour satisfaire au besoin particulier à chacun d'entretenir l'usage de la déglutition, de tout ce qui en dépend, et de ce qui la suit immédiatement (voyez la fig.).

Pour aller au devant des effets naturels, j'ai coupé des petits morceaux des bords de la base des anémones de cette espece, et je les ai conservés; plusieurs sont devenus de petites anémones, comme celles qui étoient provenues des arrachemens que s'étoient faits les anémones, et un grand nombre a péri sans produire. Ne pourroit-on pas penser d'après cette expérience, que ceux de ces morceaux qui ont produit renfermoient un petit bulbe propre à devenir anémone, et destiné à être bientôt détaché par la mère; et qu'au contraire les parties qui ont péri n'en contenoient pas, ou n'en renfermoient que de trop peu formés pour se développer après

violent amputation? I the rather incline to this opinion, having observed, that among the pieces I had cut off, those particularly succeeded which appeared interiorly replete and of a certain thickness. If so, this conjecture may possibly lead us to another. It is well known that the fresh water polypi increase by section, and that being cut into several pieces, each of these pieces becomes an animal similar to the original one. Thus a polypus being divided in 2, 4, 8, 16, or more parts, each of these parts probably contains a bulb capable of becoming another polypus. In the course of my experiments, the small pieces I cut off from the bases and robes of the anemonies did not exceed the 500th part of the animal; it is not therefore to be wondered, if many of them did not prove prolific; they probably contained none of the fertile bulbs. The reproduction of the polypus by section will then no longer be attributed to any of its rude and shapeless parts; but rather to

---

une section violente. Je suis porté à le croire, parce qu'en coupant ces morceaux, ceux où je voyois une certaine épaisseur, un certain arrondissement intérieur, sont ceux qui m'ont paru réussir. S'il en est ainsi, cette conjecture pourroit peut-être nous conduire à une autre. On sait que les polipes d'eau douce se multiplient par la section, et qu'un tel polipe étant coupé en plusieurs morceaux, chaque morceau devient un animal semblable au premier. On coupe pour cet effet un polipe en 2, 4, 8, 16 parties et plus, chacun des ces morceaux est la 16<sup>me</sup> partie de l'animal, il se peut que dans chaque 16<sup>me</sup> partie il se trouve un petite bulbe disposé à devenir un polipe. Dans mes expériences, en coupant de petits morceaux du bord de la base et de la robe de mes anémones, je ne coupois pas la 500<sup>me</sup> partie de l'animal : on ne doit donc pas s'étonner, si beaucoup de ces petits morceaux ne produisent rien; ils ne contiennent peut-être pas de bulbe. Alors la reproduction du polipe par sa partie ne paroîtroit plus due à une portion informe; mais à une

VOL. LXV. K k partie

to parts that are organized in a particular manner, to eggs, or perhaps to something more than eggs. The singular propagation of several kinds of this animal seems to favour this conjecture. In so minute an object as the fresh-water polypus, much is easily overlooked; but in the sea-anemonies, although we are far from seeing every thing, yet it is possible, even without the assistance of glasses, to discern a great deal which must escape us in the most diligent examination of the other animal. Although I speak this from experience, I do not pretend that our ideas concerning reproduction, and especially the reproduction of parts, should be confined to what is here asserted. It is known that every part of a living animal, &c. hath an organization far surpassing any idea we can form of it; that its least particles are still organized; and there is no saying how far this may extend. In seeds, &c. this organization admits of an astonishing degree of expansion,

---

partie plus particulièrement organisée, à un oeuf, ou même à quelque chose de plus qu'un oeuf. La propagation singulière de plusieurs espèces de polypes semble favoriser cette conjecture. Les polypes d'eau douce sont si petits qu'il est facile de ne pas tout appercevoir. Dans les anémones de mer, quoiqu'on soit fort éloigné de tout voir, on peut cependant, même sans le secours de la loupe, remarquer bien des choses qui échappent dans l'observation du polipe d'eau douce. Quoique je parle ici d'après l'expérience, je ne dissimulerai pas, qu'on ne doit point y restreindre ses idées à l'égard de la reproduction et surtout de la reproduction de parties. On sait que toutes les parties d'un être vivant, &c. ont une organisation qui surpasse infiniment l'idée que nous pouvons nous en faire; que ses moindres particules sont encore organisées quoiqu'infiniment petites; on ne sait pas même jusqu'où cela peut aller: cette organisation permet dans les graines, &c. des développe-  
mens

expansion, which doth not always take place in other caſes; but yet often operates when we are the leaſt aware of it. Far then from ſuffering ourſelves to be ſurprized at ſtrange effects in nature, we ſhould in ſome meaſure expect them; we ſhould be conſtantly upon the watch for them, and take pains to be apprized of them, and ſtudy to turn them to the benefit of mankind. Practitioners in ſurgery know how often they have been ſurprized by unexpected events. Such events, which commonly are only the objects of unavailing wonder, would perhaps be leſs unprofitable, if they were cauſiouſly obſerved and correctly deſcribed by perſons of knowledge, leſs fond of the marvellous than friends to truth. Lobſters are ſeen to produce new legs when deprived of their old ones; but not exactly at the places where they had been cut off. Their manner of growing is much like to that of our teeth, the interior part and very origin of the claw producing a ſmall one, &c. and this reproduction ſeems not to take place when the lobſter

is

---

mens d'une grandeur étonnante, qui n'ont pas toujours lieu ailleurs: mais qui opèrent quelquefois au moment qu'on s'y attend le moins. Loin donc de ſe laiſſer ſurprendre par des effets ſinguliers, on devroit en quelque ſorte les attendre, et être très attentif à les ſaiſir, à les connoître, et à en profiter. Ceux qui s'occupent de l'art de guérir ſavent combien ils ont été ſurpris par des effets inattendus. Ces effets, dont on ſe contente le plus ſouvent de parler avec étonnement, ne ſeroient peut-être pas ſtériles, ſi obſervés avec attention par des gens inſtruits, et auſſi peu avides du merveilleux qu'amis de la vérité, ils étoient décrits avec précision. Les pieds des écreviſſes repouſſent; mais ils ne repouſſent pas immédiatement de la partie coupée; ils pouſſent à peu près comme nos dents, c'eſt-à-dire que de l'intérieur et vers l'origine du pied, on en voit pouſſer un petit, &c. et cette reproduction ne



is deprived of the phalanx next to the body. There are worms which are known to multiply by section; although their smallness, which renders it very difficult to examine them, and the similarity they bear to other worms, whose reproduction we know to be imperfect, may furnish some grounds to doubt, whether this reproduction be so complete as hath been imagined. There is also a reproduction in the tails of lizards which cannot well be accounted for. There are experienced naturalists who still make a doubt of the multiplication of the polypi by section; and others who absolutely deny it, pretending that what hath been taken for a single animal may as probably be a sheath, the common work, and habitation of a multitude of small insects. For my part, far from doubting the possibility of such multiplications, of which I have seen positive instances in the sea-anemonies, I am not for having recourse to the particular supposition of any kind of eggs for explaining it. I believe indeed, that propagation

is

---

paroit pas avoir lieu, lorsque la phalange la plus proche du corps de l'écrevisse a été enlevée. Certains vers se multiplient par la section: on doutera peut-être que cette multiplication soit aussi complète qu'elle a paru l'être, parce qu'on n'y voit pas si bien à cause de leur petitesse, et de l'analogie qu'ils peuvent avoir avec d'autres vers dont la reproduction est imparfaite. On voit de même une reproduction qui laisse quelque chose à désirer dans la queue du lézard. Il se trouve encore des savans qui suspendent leur jugement sur la multiplication du polipe par la section; d'autres la nient absolument. Ils croient que ce qu'on a pris pour un individu pourroit être un fourreau, ouvrage et logement de certains petits insectes. Loin de douter de la possibilité de ces multiplications, dont j'ai des exemples décisifs dans les anémones de mer, je ne les assujettis pas même à des espèces d'oeufs; je crois seulement que c'est la voye la plus ordinaire; et les re-

productions

is most usually carried on by eggs. But the reproduction of very considerable and the most essential parts of my sea-anemonies convinces me, that there are wonders far beyond any thing we can imagine, which make a part of the regular established course of things. The sections of the sea-anemonies of this species differ in nothing from those of the former, except that I never yet observed an upper half producing a new basis; the contrary never yet failed. The first observation of any note, was made on the 16th of June 1773, in the presence of seven surgeons whom the singular phenomena of reproduction had induced to visit my study. The animals of this species being very large, I only operated upon young ones, and on that day cut one that was not thicker than a goose-quill. On the 30th the upper part was perfectly restored, and the fold which is seen near the upper extremity of the body of this species, appeared exactly like that I had cut off.

By

---

productions de parties essentielles et très considérables de mes anémones de mer m'obligent à croire, que les merveilles, qui font partie de l'ordre établi, vont encore plus loin que notre imagination. Les sections opérées sur cette espèce d'anémones de mer, ont donné à peu près les mêmes résultats qu'aux autres, c'est-à-dire, que coupées par la moitié du corps, elles ont repoussé une partie supérieure: cependant je n'ai point encore obtenu de base aux parties supérieures que j'en avois privées. La première opération notable se fit le 16 Juin 1773, en présence de sept chirurgiens que les phénomènes de la reproduction avoient attirés à mon cabinet. Comme cette espèce est fort grosse, je n'avois opéré que de jeunes individus: celui que je coupai alors n'étoit même que de la grosseur d'une plume à écrire. Le 30 la partie supérieure étoit totalement reproduite, et le pli qu'en voit vers l'extrémité supérieure du corps dans cette espèce paroïsoit exactement comme celui que j'avois retranché. L'exercice m'ayant rendu plus adroit, je suis parvenu à couper

d'un

By practice I arrived at such dexterity as to cut in two, at one snip of the shears, in a very straight line, an animal of this species as thick as my arm. This was performed on the 18th of October: before the end of the month new limbs appeared, of which the large ones, within the tufts, shot forth long before the others. On the 10th of December the animal began to eat, though its mouth was scarcely formed. The upper part was still alive. I tied a string round some of these anemonies whilst they were considerably extended length-ways, and pulled the noose very tight. They had the dexterity to free themselves in a few hours of this troublesome ligament, by gradually withdrawing their upper extremity: then on measuring the noose I found it not quite six lines in diameter. This species is good to eat.

Among the sea-anemonies brought me by my fishermen, I have some reason to think that I have discovered a fifth species, which seems to reside only in places from whence the sea seldom recedes. They appeared to me to be

---

d'un seul coup de ciseaux et bien droit un de ces animaux gros comme le bras. C'étoit le 18 Octobre; avant la fin du mois il reparut de nouveaux membres. Les gros qui sont en dedans des panaches paroissent longtems avant les autres. La bouche à peine formée le 10 de Décembre l'animal a mangé. La partie supérieure vivoit encore. J'ai lié de ces anémones grosses comme le bras, lorsqu'elles étoient fort allongées, et je les ai serrées fortement; elles ont eu l'adresse de se dégager en peu d'heures d'un lien aussi incommode en dépassant peu à peu leur partie supérieure. Alors j'ai mesuré le diametre du nœud; il n'avoit pas six lignes. Cette espece est bonne à manger.

Entre les anémones de mer que m'ont apporté mes pêcheurs, je crois en avoir découvert une cinquieme espece. Elle paroît résider dans les lieux que la mer ne découvre jamais. Sur ce que j'en ai vu, elle est petite comme la premiere, ses

be as small as those of the first species: their limbs, which are somewhat confusedly arranged in three rows, are also nearly the same. They have the form and the knobs of the second, and the threads of the fourth species, which latter, however, are coloured. Their mouths are round, and bordered with small reddish limbs; only one white spot is seen on one side of the mouth, whereas two of them appear in those of the third species. The middle between the mouth, and the limbs is of a greenish hue, with narrow variegated streaks which extend from the center to the circumference. The specimens I have seen were white on the superior edge of the robe, of a golden yellow in the middle, inclining to a duskier colour towards the bottom; that is to say, the ground-colour of the robe changed gradually from white at the top to brown at the bottom, passing, by imperceptible transitions, through a succession of yellow shades, partaking more or less of the colour of gold. This whole robe was speckled with light crimson spots, and no rim appeared

---

membres sont à peu près semblables, elle en a trois rangs un peu confondus. Elle a de la seconde la forme et les mamelons, et les cordons de la 4<sup>ème</sup>, mais colorés; la bouche est ronde bordée de petits membres rougeâtres; une seule marque blanche se trouve à côté de la bouche, comme on en voit deux dans la 3<sup>ème</sup> espèce. Le milieu entre la bouche et les membres est verdâtre, avec de petites rayes de couleur différente, qui vont du centre à la circonférence. Les individus que j'ai vus étoient blancs par le bord supérieur de la robe, d'un jaune doré par le milieu, et s'obscurcissant vers le bas, c'est-à-dire, que par des nuances imperceptiblement fondues, plus ou moins dorées, le fond de la couleur de cette robe passoit du blanc au brun. Toute cette robe étoit mouchetée d'un beau cramoisi clair, et il n'y avoit point de bordure

peared at the basis. These anemonies had been found on old *volutes*, called spindle-shells (*fucus brevis*). Another specimen which was found adhering to an oyster-shell was of a darker colour; but its limbs bore some resemblance to the horns of cattle: they were of a pale green, with circles of a fine dark brown, which had a very pleasing effect. These limbs appeared, at first sight, to tend towards the center, by the continuation of some semi-circles which gradually diminished.

I have said little concerning the mechanism by which these animals perform their different functions, not thinking myself sufficiently acquainted with it to satisfy even the most chastised curiosity. I leave to others the cultivation of this field, where the genius of system may find an ample range. As in physics the only real information is derived from a knowledge of facts, I thought it my duty to  
 confine

bordure à la base. Ces anémones étoient attachées sur de vieux coquillages à volute, qu'on nomme fuseaux (*fucus brevis*). Un autre individu pêché depuis, attaché sur une huitre, avoit la robe d'une couleur obscure, mais les membres paroissoient comme certaines cornes de boeuf de couleur douce, d'un blanc verdâtre, avec des cercles d'un beau brun noir qui produisoient un très bel effet. Ces membres sembloient au premier coup d'œil prolongés vers le centre, par la continuation de quelques demi cercles allant en diminuant.

J'ai dit peu de chose du mécanisme par lequel chaque faculté, chaque fonction s'opère dans les anémones de mer, ne pouvant me flâter d'en connoître assez pour satisfaire à la curiosité la mieux réglée. C'est un champ ouvert à quiconque voudra le cultiver, c'est une carrière dans laquelle l'esprit de système pourra prendre son effort. Comme en matière de physique les connoissances tirées des faits sont les seules qui nous éclairent, j'ai cru devoir m'en tenir à la simple exposition

confine myself to the bare exposition of those I have observed. Not that I am apprehensive of being thought too prolix or tedious by those for whom I am writing: I know how much they value every truth, and every new discovery: but the habit of observation and experiment, produces circumspection and a quick sense of difficulty. Besides, I find that the system-mongers, though a numerous body, have been but unsuccessful since accurate observations are come into vogue. It is this circumspection which prevents my proceeding inconsiderately from the science of bodies to that of ideas. The sea-anemonies give evident marks of sensibility; shall I thence conclude, that a soul animates them; or shall I grant that they are deprived of sensation, although favoured with the organs of it? I shall draw no conclusion, neither do I think myself at liberty to amplify upon that subject.

My

sition de ceux que j'ai eu lieu d'observer. Ce n'est cependant ni la crainte de paroître diffus, ni celle d'ennuier ceux pour lesquels j'écris qui m'a retenu; toute vérité leur est chère, toute découverte leur est précieuse: mais l'habitude de contempler et d'expérimenter rend circonspect, et fait appercevoir les difficultés: d'ailleurs les faiseurs de systèmes, quoiqu'en grand nombre, ne sont pas heureux depuis qu'on observe bien. Cette même circonspection ne m'a pas permis de passer inconsiderément de la science des corps dans celle des idées. Les anémones de mer me paroissent douées de sensations, en conclurai-je qu'un esprit les anime, ou quelles sont privées du sentiment dont elles ont les organes? Je n'en concluerai rien, et je crois même mon mémoire peu susceptible d'amplifications dans cette partie.

My very earliest observations shewed me that the sea-anemonies feel and prognosticate within doors the different changes of temperature in the atmosphere. I had not leisure at that time to form tables of their various indications; but I have since done it. This fact, if applied to practice, might be of use in the formation of a sea-barometer, an object of no small importance, which several ingenious men have hitherto endeavoured in vain to furnish us with. I should prefer the anemonies of the third species for this purpose, their sensation being very quick; they are also easily procured, and may be kept without nourishment. Five of them may be put in a glass vessel, four inches wide and as many in depth, in which they will soon cleave to the angle formed by the sides and the bottom. The water must be renewed every day, and, as they do not require a great quantity of it, as much may be fetched from the sea (if they be kept on land) as will supply them for several days; its settling  
some

---

Les anémones de mer ressentent d'avance, et annoncent dans le cabinet les effets de la température; c'est ce que j'ai eu lieu de remarquer dès mes premières observations; j'étois trop occupé alors pour en dresser des tables: je l'ai fait depuis. Ce fait mis en pratique pourra suppléer le baromètre marin, dont plusieurs savans ont fait inutilement la recherche jusqu'ici: ce ne seroit pas un petit avantage. Les anémones de la troisième espèce m'ont paru plus propres que les autres à cet effet, parcequ'elles sont très sensibles. Il est aisé de s'en procurer, de les loger, de les conserver sans les nourrir &c. On mettra cinq de ces anémones dans un vase de verre d'environ quatre pouces de diamètre sur autant de hauteur, elles s'y attacheront à l'angle que forme le fond avec les parois. Il faut renouveler l'eau tous les jours. Comme la quantité d'eau nécessaire n'est pas grande, si on est à terre, on peut en faire prendre à la mer, pour plusieurs jours; elle n'en  
fera

some time will only improve it. I commonly renew the water every evening, because if that change makes any impression on the animals, it will not interfere with the principal observation, which, when I was forming my tables, I always took at a time equally distant from the renewals of the water. If the anemonies be at that time shut and contracted (as in fig. 14. of my first essay) I have reason to apprehend an approaching storm; that is, high winds and a rough agitated sea. When they are all shut, but not remarkably contracted, they forebode a weather somewhat less boisterous, but still attended with gales and a rough sea. If they appear in the least open, or alternately and frequently opening and closing, they indicate a mean state both of winds and waves. When they are quite open, I expect tolerable fine weather and a smooth sea. And lastly, when their bodies are considerably extended and their limbs divergent, they surely prognosticate fixed fair weather and a very calm.

---

fera que meilleure étant reposée. Je suis dans l'usage de renouveler l'eau le soir, parceque si ce changement d'eau fait quelque impression aux anémones, il est alors compté pour rien; et quant à l'observation la plus notable, lorsque je dressois mes tables, je la faisois à la moitié du jour. Si les anémones sont toutes fermées, et retirées sur elles mêmes, comme dans la figure 14 de mon premier mémoire, j'ai lieu de craindre qu'il ne survienne quelque tempête, c'est à dire gros vent, la mer fort agitée, et troublée. Lorsqu'elles sont toutes fermées sans être retirées, celz annonce un temps un peu moins facheux, comme grand frais de vent, la mer agitée, &c. Si je vois mes anémones entrouvertes ou s'ouvrant et se fermant de moment à autre, c'est pour la mer et pour le vent un état moyen. Quand elles sont ouvertes, j'attends un temps assez beau, la mer sera peu agitée; enfin ont-elles le corps allongé, et les membres très étendus, c'est le présage d'un beau fixe et



calm sea. There are times when some of the anemonies are open and others shut; the number must then be consulted; the question is decided by the majority. Accuracy in these observations is in a great measure acquired by habit. At sea the glass may be swung, in the same manner as the compass, by suspending it between two concentric circles, moving upon pivots that cross each other at right angles, so that the rolling of the ship may agitate the water as little as possible. The anemonies that are used as *barometers* should not be fed, for then the quantity of nourishment might influence their predictions. Anemonies of this and of the first species live and do well for several years, without taking any other food but what they find diffeminated in the sea water; but should a respite of some days be granted them, they might then be fed with some pieces of muscles or soft fish, and thus restored to their original vigour. When-

every

---

d'une mer très calme. Il y a des momens où des anémones sont ouvertes et d'autres fermées; alors on conclut par le plus grand nombre, on va pour ainsi dire à la pluralité des voix: l'habitude aide à se décider. En mer on pourra suspendre le verre, comme une boussole ou compas de mer, entre deux cercles concentriques ou balanciers, avec des pivots qui se croisent à angles droits, afin que le tangage et les roulis du vaisseau n'agitent l'eau que le moins, qu'il sera possible. Il ne faut point nourrir les anémones qui servent de baromètre: car alors elles pourroient se fermer ou se tenir ouvertes, à raison de la nourriture qu'elles auroient prise. Une anémone de cette espece, et de la premiere, vit très bien et en assez bon-état, pendant plusieurs années, sans prendre d'autre nourriture que celle qu'elle trouve diffeminée dans l'eau de la mer; cependant, si on avoit lieu de leur accorder une vacance de quelques jours, on pourroit les nourrir, et conséquemment leur redonner leur première vigueur par quelques morceaux de moule ou de poisson mou. Lorsque

le

ever the vessel is sullied by the sediments of salts, slime, the first shoots of sea-plants, &c. it may, on changing the water, be cleansed by wiping it with a soft hair-pencil, or even with the finger, carefully avoiding, however, to rub or press hard upon the anemonies. Should any of them drop off during this operation, they may be left at liberty, for they will soon, of their own accord, fix themselves to some other place. Should any of them die, which will soon be discovered by the milky colour of the water; and an offensive smell on changing it, it must be taken out, and on the first opportunity, another of the same species be put in its place; those of a moderate size are the most eligible. Such barometers might be applied as ornaments; for as they can be made by the persons who are to use them, they might consult their own taste in the choice of the colours and shades of the limbs. A few anemonies of the first species might also be added, for as those of the third generally take up their habitation

---

le vaîe sera sali par le dépôt des sels, le limon, ou par des commencemens de plantes marines, &c. on pourra en changeant l'eau, le nettoyer avec un pinceau doux, ou même avec le doigt, observant de ne passer que légèrement le doigt sur les anémones pour ne les pas comprimer. Si une anémone se détache, on la laissera en liberté, elle s'attachera en un autre lieu. Si elle vient à périr, ce qu'on reconnoitra par une très mauvaise odeur en changeant l'eau, et parceque cette eau devient un peu laiteuse, on retirera l'anémone, et on lui en substituera à la première occasion une autre toujours de même espèce et de moyenne grosseur; ce sont les meilleures. Ce baromètre est agréable à la vue, car on peut choisir des anémones dont les membres soient d'une couleur haute, douce, ou variée selon son gout, puis qu'on le construit soi même : on peut encore y ajouter plusieurs anémones de la première espèce. Comme celles de la troisième se tiennent vers le fond.

tion at the bottom of the vessel, these commonly keep near the surface of the water; and their various bay, crimson, dark, purple, green, and other colours, would add no small lustre to the variegated object: nor should their prognostications concerning the temperature of the air be altogether neglected. Perhaps they may at first produce young ones, which, if attended to, will be seen to encrease gradually. Should the anemones of this first species not be used for observations, they might be fed and thus kept in a most flourishing condition. The form, size, and ornaments of this *barometer* may also be accommodated to any place or situation. The only inconvenience attending it would be the changing of the water every day; but even this amounts to no more than winding up our watches: neither is it absolutely necessary to do it exactly at the same hour; nor is there any harm in the common practice of leaving the same water for several days, unless, indeed, it be inclined to

---

du vase, celles-ci aiment à être à la surface de l'eau, leur couleur brun-rouge, cramoisi, pourpre, foncé, vert, &c. fera un bel effet, et leur avis sur la température n'est pas tout-à-fait à négliger. Elles pourront dans le commencement donner des petits, qu'on verra grossir par la suite. Si on ne vouloit pas tenir compte des indications des anémones de cette première espèce, en les nourrissant elles ne seroient que plus belles. Ce baromètre est encore fort commode, aisé à placer, et susceptible de toute espèce de forme et de décoration. Il peut être aussi grand et aussi petit qu'on veut. La seule incommodité est de changer l'eau chaque jour, ce qui n'est guère plus difficile que de remonter sa montre, encore n'est-on pas strictement obligé de le faire à la même heure, et il n'y a aucun inconvénient dans l'usage ordinaire de laisser la même eau pendant plusieurs jours, à moins qu'elle ne soit exposée à se corrompre par une chaleur extrême, ou par quelque autre inconvénient

peu

to corruption, by being exposed to a great deg. of heat, or by some other uncommon accident. If ever the *barometer* was to be removed, the vessel might be left empty for a day or two, without any considerable detriment to the anemonies; they will contract, and continue fixed to the sides, and resume their wonted manœuvres, as soon as fresh sea water is poured upon them. One necessary caution is, to dispose of the barometer in a place where, as nearly as possible, the same degree of light prevails throughout the day: for it must be observed, that these animals, on being suddenly removed out of the dark into a strong light will frequently contract in an instant, though they will soon recover after their usual form. As the simple barometer, the Torricellian tube, which is the best of all, doth not always agree with the visible state of the local atmosphere, and that frequently its indications are far from being exact; it cannot be expected that the sea-anemonies should excel in this respect, and that their indications

---

peu ordinaire. On peut même vider l'eau pour transporter le baromètre ou l'on veut, pendant un jour entier et plus, sans que les anémones périssent; elles se retireront sur elles mêmes, et se tiendront attachées. On leur procurera de nouvelle eau de mer, et elles reprendront leur manœuvre ordinaire. Une attention nécessaire c'est de placer autant qu'on le peut le baromètre, d'anémones dans un lieu où il regne pendant tout le jour une lumière à peu près égale: car on doit se souvenir, que souvent les anémones qui passent subitement d'un lieu obscur à une grande lumière se ferment; mais elles reprennent bientôt l'état où elles auroient resté. Comme le baromètre simple, le meilleur qui existe, le tube de Toricelli, n'est pas toujours parfaitement d'accord avec l'état visible de l'atmosphère du lieu où l'on observe, et qu'assez souvent ses indications ne sont pas d'une justesse rigoureuse, on ne doit pas s'attendre que les anémones de mer rem-

plissent

indications should always coincide with the above instrument. But as, with all its imperfections, the barometer above mentioned is of great utility, why should not also this, formed of sea-anemonies, be adopted notwithstanding their little irregularities. These are not the only animals that are influenced by the approaching changes of weather; the petrel, different sorts of gulls, the scink, the leech, the little fish of the Danube, the cetaceous animals, the sea-urchins, &c. are all said to feel and prognosticate those variations.

Amidst the more important objects of utility and necessity, every pleasurable improvement should not be totally neglected. I shall not therefore scruple to communicate what ideas have occurred to me, tending to this purpose. It appears to me, that the large anemonies, and even the smaller ones, being arranged in an advantageous manner, and properly grouped with acorns, barnacles, sea-weeds, &c. might furnish architecture, painting,

---

plissent mieux cet objet, ni qu'elles soient toujours d'accord avec lui; et comme avec ses inexactitudes, le baromètre est un bon instrument, on pourra se servir de celui d'anémones, indépendamment de ses petits caprices. Ces animaux ne sont pas les seuls qui ressentent l'influence de la température prochaine, l'oiseau-tempête, le petrel, les poulets de mer, le mabouja, la sangsue, le petit poisson du Danube, les cétacées, les ourfins, &c. passent aussi pour la ressentir, et même pour l'annoncer.

Les vues d'utilité, de nécessité, ne doivent point nous faire négliger celles d'agrément: je ne craindrai donc pas de dévoiler ici celles que j'ai sur ce point. Il me semble que les grandes et même les petites anémones de mer prises dans des positions variées les plus avantageuses, et artistement groupées entr'elles ou avec les glands de mer, les conques anatifères, les plantes marines, &c; fourniroient

ing, sculpture, and all the arts dependant upon drawing, with a variety of elegant and pleasing ornaments, especially in naval and hydraulic architecture, in which, contrary to all the maxims of true taste, and regardless of probability, chimerical ornaments are often introduced, which have no manner of relation or affinity with the structures they are intended to decorate. They are often seen affixed to substances to which the things they represent have little or no tendency to adhere; whereas the sea-anemonies spontaneously cling to every substance in and even out of water, and might therefore being grouped into picturesque festoons, be with great propriety substituted to the flowery garlands and other such ornaments which preposterously decorate the sterns of ships and the frizes on barges, and not seldom the most stately edifices, such as bridges, sluices, &c.

Expla-

---

roient à l'architecture, à la peinture, à la sculpture, et à tous les arts qui ont le dessein pour base, de beaux ornemens, sur tout pour l'architecture navale, et même pour l'architecture hydraulique, où, contre les règles du goût, contre la vraisemblance, on employe souvent des ornemens chimériques et peu analogues au sujet. On groupe, on attache ces objets à des lieux peu propres, peu disposés à les recevoir; au contraire les anémones de mer s'attachent à tout ce qui se trouve dans l'eau et même hors de l'eau. Elles pourroient être substituées aux rozettes, former des groupes, des especes de guirlandes pittoresques dans la décoration des poupes des vaisseaux, dans les frises qui ornent les canôts, et dans quelques édifices hydrauliques, comme les ponts, les écluses, &c.

Explanation of TAB. VI. which represents a group of sea-anemonies of the fourth species, adhering to an oyster-shell.

Fig. 1. A contracted anemony of the natural and middling size adhering to the oyster-shell. Fig. 2. Anemonies united to the same trunk, compared in the essay to the letter Y. At its basis a little shred appears ready to be torn off in order to become a new anemony. Fig. 3. and 4. Two young anemonies moderately expanded, in the middle of which the mouths appear. Fig. 5. An anemony, somewhat more grown, on which the projecting rim appears. Fig. 6. Eight small anemonies, two of which adhere together, as do also two others, which, however, are upon the point of being separated by the contraction of the part that unites them. Other small anemonies, of different sizes, are seen upon the oyster-shell.

---

Explication des Figures. La planche représente un groupe d'anémones de mer de la quatrième espèce, attachées sur une huître.

Fig. 1. Anémone de grosseur naturelle et moyenne attachée sur l'huître et fermée. Fig. 2. Anémones réunies sur une même tige, elle est désignée dans le mémoire par la forme d'un Y. On voit au bord de sa base un petit lambeau prêt à en être détaché pour devenir une anémone. Fig. 3. et 4. sont deux jeunes anémones plus ou moins ouvertes, au milieu desquelles on remarque la bouche. Fig. 5. Anémone un peu plus forte, dont on voit le bourlet. Fig. 6. Ce sont huit petites anémones, dont deux sont adhérentes ensemble; deux autres le sont aussi, mais prêtes à se séparer par l'étranglement de la partie qui les unit. On remarque sur l'huître d'autres anémones plus ou moins petites.



XX





XXI. *Account of the Sea-Cow, and the Use made of it.*  
*By Molineux Shuldharn, Esquire.*

Redde, Mar. 2, 1775. **T**HE sea-cow is a native of the Magdalen Islands, St. John's, and Anticosti in the Gulph of St. Lawrence. They resort very early in the spring to the former of these places, which seems to be by nature particularly adapted to the wants of these animals, abounding with (a) clams of a very large size, and the most convenient landing places, called Echouries. Here they crawl up in great numbers, and sometimes remain for fourteen days together without food, when the weather is fair; but on the first appearance of rain, they immediately retreat to the water with great precipitation. They are, when out of the water, very unweildy, and move with great difficulty. They weigh from 1500 to 2000 pounds, producing, according to their size, from one to two barrels of oil, which is boiled out of a fat substance that lies between the skin and the flesh. Immediately on their arrival they calf, and engender again about two months after; so that they carry their young about nine months. They never have more than two at a time, and seldom more than one.

(a) A shell-fish resembling a scallop.

The echouries are formed principally by nature, being a gradual slope of soft rock, with which the Magdalen islands abound, about 80 or 100 yards wide at the water side, and spreading so as to contain, near the summit, a very considerable number. Here they are suffered to come on shore and amuse themselves for a considerable time, till they acquire a boldness, being at their first landing so exceedingly timid as to make it impossible for any person to approach them. In a few weeks they assemble in great numbers; formerly, when undisturbed by the Americans, to the amount of seven or eight thousand; and the form of the echourie not allowing them to remain contiguous to the water, the foremost ones are insensibly pushed above the slope. When they are arrived to a convenient distance the fishermen, having provided the necessary apparatus, take the advantage of a sea wind, or a breeze blowing rather obliquely on the shore, to prevent the smelling of these animals (who have that sense in great perfection, contributing to their safety), and with the assistance of very good dogs, endeavour in the night time to separate those that are the farthest advanced from those next the water, driving them different ways. This they call making a cut, and is generally looked upon to be a most dangerous process, it being impossible to drive them in any particular direction, and difficult to avoid them; but as they are advanced above the slope of the echourie, the darkness of the night deprives them of every direction to the water, so that they stray about and are killed at leisure, those

that are nearest the shore being the first victims. In this manner there has been killed fifteen or sixteen hundred at one cut. They then skin them, and take off a coat of fat that always surrounds them, which they dissolve by heat into oil. The skin is cut into slices of two or three inches wide, and exported to America for carriage traces, and to England for glue. The teeth is an inferior sort of ivory, and is manufactured for the same purposes, but very soon turns yellow.

XXII. *The Procefs of making Ice in the Eaft Indies. By Sir Robert Barker, F. R. S. in a Letter to Dr. Brocklesby.*

TO DR. RICHARD BROCKLESBY, F. R. S.

SIR,

Spring-Gardens,  
March 2, 1775.

Redde, Mar. 9,  
1775.

THE procefs of making ice in the Eaft Indies having become a fubject of speculation, I beg permiffion to prefent you with the method by which it was performed at Allahabad, Mootegil, and Calcutta, in the Eaft Indies, lying between  $25\frac{1}{2}$  and  $23\frac{1}{2}$  degrees of North latitude. At the latter place I have never heard of any perfons having difcovered natural ice in the pools or cifterns, or in any waters collected in the roads; nor has the thermometer been remarked to defcend to the freezing point; and at the former very few only have difcovered ice, and that but feldom. But in the procefs of making ice at thefe places it was ufual to collect a quantity every morning, before Sun-rife (except in fome particular kinds of weather, which I fhall fpecify in the fequel), for near three months in the year: *viz.* from December till February.

The ice-maker belonging to me at Allahabad (at which place I principally attended to this enquiry) made

a fuf-

a sufficient quantity in the winter for the supply of the table during the summer season. The methods he pursued were as follows: on a large open plain, three or four excavations were made, each about thirty feet square and two deep; the bottoms of which were strewed about eight inches or a foot thick with sugar-cane, or the stems of the large Indian corn dried. Upon this bed were placed in rows, near to each other, a number of small, shallow, earthen pans, for containing the water intended to be frozen. These are unglazed, scarce a quarter of an inch thick, about an inch and a quarter in depth, and made of an earth so porous, that it was visible, from the exterior part of the pans, the water had penetrated the whole substance. Towards the dusk of the evening, they were filled with soft water, which had been boiled, and then left in the afore-related situation. The ice-makers attended the pits usually before the Sun was above the horizon, and collected in baskets what was frozen, by pouring the whole contents of the pans into them, and thereby retaining the ice, which was daily conveyed to the grand receptacle or place of preservation, prepared generally on some high dry situation, by sinking a pit of fourteen or fifteen feet deep, lined first with straw, and then with a coarse kind of blanketing, where it is beat down with rammers, till at length its own accumulated cold again freezes and forms one solid mass. The mouth of the pit is well secured from the exterior air with straw and blankets, in the manner of the lining, and a thatched roof is thrown over the whole. It is here necessary to  
 remark,

From these circumstances it appears, that water, by being placed in a situation free from receiving heat from other bodies, and exposed in large surfaces to the air, may be brought to freeze when the temperature of the atmosphere is some degrees above the freezing point on the scale of FAHRENHEIT's thermometer; and by being collected and amassed into a large body, is thus preserved, and rendered fit for freezing other fluids, during the severe heats of the summer season. In effecting which there is also an established mode of proceeding; the sherbets, creams, or whatever other fluids are intended to be frozen, are confined in thin silver cups of a conical form, containing about a pint, with their covers well luted on with paste, and placed in a large vessel filled with ice, salt-petre, and common salt, of the two last an equal quantity, and a little water to dissolve the ice and combine the whole. This composition presently freezes the contents of the cups to the same consistency of our ice creams, &c. in Europe; but plain water will become so hard as to require a mallet and knife to break it. Upon applying the bulb of a thermometer to one of these pieces of ice, thus frozen, the quicksilver has been known to sink two or three degree below the freezing point: so that from an atmosphere apparently not mild enough to produce natural ice, ice shall be formed, collected, and a cold accumulated, that shall cause the quicksilver to fall even below the freezing point. The promising advantages of such a discovery could alone induce the Asiatic (whose principal study is the luxuries of life, and this may well be called such, when

when I have often regaled with ices when the thermometer has stood at 112°), to make an attempt of profiting by so very short a duration of cold during the nights in these months, and by a well-timed and critical contrivance of securing this momentary degree of cold, they have procured to themselves a comfortable refreshment as a recompence, to alleviate, in some degree, the intense heats of the summer season, which, in some parts of India, would be scarce supportable, but by the assistance of this and many other inventions.

Accompanying I beg leave to offer you some observations, I made in the year 1767<sup>(b)</sup>, in the province of Allahabad, on the temperature of the weather, which will serve to elucidate the extraordinary and sudden changes incident to that part of Asia. Also some remarks on the weather during my voyage to England, particularly specifying the situation we were in when the observations were made.

I am, SIR, with regard,

Yours

Your most obedient humble servant,

ROBERT BARKER.

(b) See a general account of these observations Article XVIII.



XXIII. *Of the House-Swallow, Swift, and Sand-Martin*.\*

By the Rev. Gilbert White, in *Three Letters to the Hon. Daines Barrington, F. R. S.*

## LETTER I.

## OF THE HOUSE-SWALLOW.

DEAR SIR, Selborne, Jan. 29, 1775.

Redde, Mar. 16, 1775. **T**HE house-fswallow, or chimney-fswallow, is undoubtedly the first comer of all the British *hirundines*; and appears in general on or about the 13th of April, as I have remarked from many years observation. Not but now and then a straggler is seen much earlier; and in particular, when I was a boy, I observed a fswallow, for a whole day together, on a sunny warm Shrove-tuesday, which day could not fall out later than the middle of March, and often happens early in February. It is worth remarking, that these birds are seen first about lakes and mill-ponds: and it is also very particular, that if these early visitors happen to find frost and snow, as was the case in the two dreadful springs of 1770 and 1771, they immediately withdraw for a time. A circumstance this much more in favour

\* See an account of the House-Martin by the same gentleman, in *Phil. Trans.* vol. LXLV. p. 196.

hiding than migration; since it is much more probable, that a bird should retire to its *hybernaculum* just at hand, than return for a week or two only to warmer latitudes. This swallow, though called the chimney-swallow, by no means builds altogether in chimneys; but often within barns and out-houses, against the rafters: and so she did in Virgil's time:

— “ *Ante*

“ *Garrula quam tignis nidus suspendat hirundo.*”

In Sweden she builds in barns, and is called *ladu svala*, the barn-swallow. Besides, in the warmer parts of Europe, there are no chimneys to houses except they are English-built. In these countries she constructs her nest in porches and gateways, galleries, and open halls. Here and there a bird may affect some odd, peculiar place; as we have known a swallow build down the shaft of an old well, through which chalk was formerly drawn up, for the purposes of manure; but in general with us, this *hirundo* breeds in chimneys, and loves to haunt those stacks where there is a constant fire, no doubt for the sake of warmth. Not that it can subsist in the immediate shaft where there is a fire; but prefers one adjoining to that of the kitchen, and disregards the perpetual smoke of that funnel, as I have often observed with some degree of wonder. Five or six or more feet down the chimney does this little bird begin to form her nest about the middle of May, which consists, like that of the house-martin, of a crust or shell, composed of dirt and mud, mixed with short pieces of straw, to render it tough and permanent;

permanent; with this difference, that whereas the shell of the martin is nearly hemispheric, that of the swallow is open at the top, and like half a deep dish. This nest is lined with fine grasses and feathers, which are often collected as they float in the air. Wonderful is the address which this adroit bird shews all day long in ascending and descending, with security, through so narrow a pass. When hovering over the mouth of the funnel, the vibrations of her wings, acting on the confined air, occasion a rumbling like thunder. It is not improbable, that the dam submits to this inconvenient situation so low in the shaft, in order to secure her broods from rapacious birds, and particularly from owls, which frequently fall down chimnies, perhaps in attempting to get at these nestlings. The swallow lays from four to six white eggs, dotted with red specks; and brings out her first brood about the last week in June, or the first week in July. The progressive method by which the young are introduced into life is very amusing. First they emerge from the shaft with difficulty enough, and often fall down into the rooms below. For a day or so they are fed on the chimney-top; and then are conducted to the dead leafless bough of some tree, where, sitting in a row, they are attended with great assiduity, and may then be called *perchers*. In a day or two more they become *flyers*, but are still unable to take their own food; therefore they play about near the place where the dams are hawking for flies; and when a mouthful is collected, at a certain signal given, the dam and the nestling advance

rising

rising towards each other, and meeting at an angle; and the young one all the while uttering such a little quick note of gratitude and complacency, that a person must have paid very little regard to the wonders of nature that has not often remarked this feat. The dam betakes herself immediately to the business of a second brood, as soon as she is disengaged from her first; which at once associates with the first broods of house-martins, and with them congregates, clustering on sunny roofs, towers, and trees. This *hirundo* brings out her second brood towards the middle and end of August. All the summer long is the swallow a most instructive pattern of unwearied industry and affection! For, from morning to night, while there is a family to be supported, she spends the whole day in skimming close to the ground, and exerting the most sudden turns and quick evolutions. Avenues and long walks under hedges, pasture fields and mown meadows where cattle graze, are her delight, especially if there are trees interspersed; because in such spots insects most abound. When a fly is taken a smart snap from her bill is heard, resembling the noise at the shutting of a watch-case; but the motion of the mandibles is too quick for the eye. The swallow, probably the male bird, is the *excubitor* to the house-martins, and other little birds, announcing the approach of birds of prey: for as soon as an hawk appears, with a shrill alarming note, he calls all the swallows and martins about him, who pursue in a body, whilst they buffet and strike their enemy till they have driven him from the village, dart-

ing

every circumstance that does not immediately respect self-preservation, or lead at once to the propagation or support of their species. I am, with all respect, &c.

---

## L E T T E R II.

### Of the SWIFT, or BLACK MARTIN.

DEAR SIR,

Selborne, Sept. 28, 1774.

**A**S the *Swift* or *Black Martin* is the largest of the British *hirundines*, so it is undoubtedly the latest comer: for I remember but one instance of its appearing before the last week in April; and in some of our late frosty, harsh springs it has not been seen till the beginning of May. This species usually arrives in pairs. The swift, like the sand-martin, is very defective in architecture, making no crust or shell for its nest; but forming it of dry grasses and feathers, very rudely and inartificially put together. With all my attention to these birds I have never been able to discover one in the act of collecting or carrying in materials: so that I have suspected (since their nests are exactly the same) that they sometimes usurp upon the house-sparrows and expel them, as sparrows do the house and sand-martin; well remembering that I have seen them squabbling together

gether at the entrance of their holes, and the sparrows up in arms and much disconcerted at these intruders. And yet I am assured, by a nice observer in such matters, that they do collect feathers for their nests in Andalusia; and that he has shot them with such materials in their mouths. Swifts, like sand-martins, carry on the business of nidification quite in the dark, in crannies of towers and steeples, and upon the tops of the walls of churches under the roof; and therefore cannot be so narrowly watched as those species that build more openly; but from what I could ever observe, they begin nesting about the middle of May, and I have remarked, from eggs taken, that they have sat hard by the ninth of June. In general they haunt high buildings, churches, and steeples, and build only in such; yet in this village some pairs frequent the lowest and meanest cottages, and educate their young under those thatched roofs. We remember but one instance where they bred out of buildings; and that is in the sides of a deep chalk-pit near the town of Odiham in this county\*, where we have seen many pairs entering the crevices and skimming and squeaking round the precipices. As I have regarded these amusive birds with great attention, if I should advance something new and peculiar with respect to them, and different from all other birds, I might perhaps be credited; especially as my assertion is the result of many years exact observation. The fact that I would advance is, that swifts *tread* or copulate on the wing: and I could

\* *Viz.* Hampshire.

with any nice observer, that is startled at this supposition, to use his own eyes, and I think he will soon be convinced. In another class of animals, *videlicet*, the *insect*, nothing is so common as to see the different species of many *genera* in conjunction as they fly. The swift is almost continually on the wing; and as it never settles on the ground, on trees, or roofs, would seldom find opportunity for amorous rites, was it not enabled to indulge them in the air. If any person would watch these birds on a fine morning in May, as they are sailing round at a great height from the ground, he would see every now and then one drop on the back of another, and both of them sink down together for many fathoms, with a loud piercing shriek. This I take to be the juncture when the business of generation is carrying on. As the swift eats, drinks, collects materials for its nest, and, as it seems, propagates on the wing; it appears to live more in the air than any other bird, and to perform all functions there, save those of sleeping and incubation. This *hirundo* differs widely from its congeners, in laying invariably but *two eggs* at a time, which are milk-white, long, and peaked at the small end; whereas the other species lay at each brood from *four* to *six*. It is a most alert bird, rising very early, and retiring to rest very late, and is on the wing, in the height of summer, at least sixteen hours. In the longest days it does not withdraw to rest till a quarter before nine in the evening, being the latest of all day-birds. Just before they retire, whole groups of them assemble in the air, and squeak and shoot about  
with

with wonderful rapidity. But this bird is never so much alive as in sultry, thundry weather, when it expresses great alacrity, and calls forth all its powers. In hot mornings, several getting together in little parties, dash round the steeples and churches, squeaking as they go in a very clamorous manner: these, by nice observers, are supposed to be males serenading their sitting hens; and not without reason, since they never squeak till they come close to the walls or caves; and since those within utter at the same time a little inward note of complacency. When the hen has sat hard all day, she rushes forth just as it is almost dark, when she stretches and relieves her weary limbs, and snatches a scanty meal for a few minutes, and then returns to her duty of incubation. Swifts when wantonly and cruelly shot, while they have young, discover a little lump of insects in their mouths, which they pouch and hold under their tongue. In general they feed in a much higher district than the other species; a proof that gnats and other insects do also abound to a considerable height in the air. They also range to great distance, since loco-motion is no labour to them, who are endowed with such vast powers of wing. Their powers seem to be in proportion to their levers; and their wings are longer in proportion than those of almost any other bird. When they mute, or ease themselves, in flight, they raise their wings, and make them meet over their heads. At some certain times in the summer I had remarked, that swifts were hawking low for hours together, over pools and streams; and could not



not help enquiring into the object of their pursuit, that induced them to descend so much below their usual range. After some trouble, I found that they were taking *phryganeæ*, *ephemeræ*, and *libellulæ* (cadew-flies, may-flies, and dragon-flies), that were just emerged out of their aurelia-state. I then no longer wondered that they should be so willing to stoop for a prey, that yielded them such plentiful and succulent nourishment. They bring out their young about the middle or latter end of July; but as these never become perchers, nor, that ever I could discern, are fed on the wing by their dams, the coming forth of the young is not so notorious as in the other species. On the 30th of June last, I untiled the eaves of a house where many pairs build, and found in each nest only *two* naked, squab *pulli*. On the eighth of July I repeated the same enquiry, and found they had made very little progress towards a fledged state; but were still naked and helpless. From whence we may conclude, that birds, whose way of life keeps them perpetually on the wing, would not be able to quit their nest till the end of the month. Swallows and martins, that have numerous families, are continually feeding them every two or three minutes; while swifts, that have but two young to maintain, are much at their leisure, and do not attend on their nests for hours together. Sometimes they pursue and strike at hawks that come in their way; but not with that vehemence and fury that swallows express on the same occasion. They are out all day long in wet days, feeding about, and disregarding still rain: from whence

whence two things may be gathered; first, that many insects abide high in the air, even in rain; and next, that the feathers of these birds must be well preened to resist so much wet. Windy, and particularly windy weather with heavy showers, they dislike; and on such days withdraw, and scarce ever are seen. There is a circumstance respecting the *colour* of swifts, which seems not to be unworthy our attention. When they appear in the spring they are all over of a glossy, dark, soot-colour, except their chins, which are white; but by being all day long in the sun and air they become quite weather-beaten and bleached before they depart; and yet they return glossy again in the spring. Now if they pursue the sun into lower latitudes, as some suppose, in order to enjoy a perpetual summer, why do they not return bleached? Do they not rather, perhaps, retire to rest for a season, and at that juncture moult and change their feathers, since all other birds are known to moult soon after the season of breeding? Swifts are very anomalous in many particulars, dissenting from all their congeners not only in the number of their young, but in breeding but *once* in a summer; whereas all the other British *hirundines* breed invariably *twice*. It is past all doubt, that swifts can breed but once, since they withdraw in a very short time after the flight of their young, and some time before their congeners bring out their second broods. We may here remark, that as swifts breed but *once* in a summer, and only *two* at a time, and the other *hirundines* twice, the latter, who lay from *four* to *six*

fat eggs, increase at an average five times as fast as the former. But in nothing are swifts more singular than in their early retreat. They retire, as to the main body of them, by the 10th of August, and sometimes a few days sooner; and every straggler invariably withdraws by the 20th, while their congeners, all of them, stay till the beginning of October, many of them stay all through the month, and some occasionally to the beginning of November. This early retreat is mysterious and wonderful, since that time is often the sweetest season in the year. But what is more wonderful, they retire still earlier in the most southerly parts of Andalusia, where they can be no ways influenced by any defect of heat; or, as one might suppose, by any defect of food. Are they regulated in their motions with us by a failure of food? or by a propensity to moulting? or by a disposition to rest after so rapid a life? or by what? This is one of those incidents in natural history that not only baffles our searches, but almost eludes our guesses! These *hirundines* never perch on trees or roofs, and so never congregate with their congeners. They are fearless while haunting their nesting places; are not to be scared with a gun; and are often beaten down with poles, as they stoop to go under the eaves. They are also much infested with those pests to the whole genus, called *bippoboscæ birundinis*; and often wriggle and scratch themselves in their flight to get rid of these clinging annoyances. Swifts are no songsters, and have only one harsh screaming note; yet there are ears to which that note is not displeasing from

an agreeable association of ideas, since that note never occurs but in the most lovely summer weather. They never settle on the ground but through accident, and when down can hardly rise, on account of the shortness of their legs and the length of their wings: neither can they walk, but only crawl; but they have a strong grasp with their feet, by which they cling to walls. Their bodies being flat, they can enter into a very narrow crevice; and where they cannot pass on their bellies, they will turn up edgewise. The particular formation of the swift's foot discriminates that bird from all the British *hirundines*, and indeed from all other known birds, the *hirundo melba*, or great white-bellied swift of Gibraltar, excepted; for it is so disposed as to carry *omnes quatuor digitos anticos*. Besides, the least toe, which should be the back toe, consists only of one bone alone; and the other three only of two apiece. A construction most rare and peculiar; but nicely adapted to the purposes in which their feet are employed. This, and some peculiarities attending the nostrils and under mandible, have induced a discerning naturalist<sup>(a)</sup> to suppose, that this species might constitute a *genus per se*. In London, a party of swifts frequent the Tower, playing and feeding over the river just below the bridge: others haunt some of the churches of the Borough next the fields; but do not venture, like the house-martin, into the close, crowded parts of the town. The Swedes have bestowed a very pertinent

(a) JOHN ANTONY SCOPOLI, of Carniola, M. D.

name on this swallow, calling it *ring swala* from the perpetual *rings* or circles that it takes round the scene of its nidification. I am, &c.

---

### L E T T E R III.

Of the SAND-MARTIN, or BANK-MARTIN.

DEAR SIR,

Selborne, Feb. 26, 1774.

THE Sand-martin, or Bank-martin, is by much the least of any of the British *birundines*, and as far as ever we have seen, the smallest known *birundo*; though BRISSON asserts that there is one much smaller, and that is the *birundo esculenta*. But it is much to be regretted, that it is scarce possible for any observer to be so full and exact as he could wish, in reciting the circumstances attending the life and conversation of this little bird; since it is *fera natura*, at least in this part of the kingdom, disclaiming all domestic attachments, and haunting wild heaths and commons where there are large lakes, while the other species, especially the swallow and house-martin, are remarkably gentle and domesticated, and never seem to think themselves safe but under the protection of man. Here are in this parish, in the sand-pits and banks of the lakes of Woolmer Forest, several colonies of these birds; and yet they are never seen in the village, nor do they at all frequent the cottages

tages that are scattered about in that wild district. The only instance, I ever remember, where this species haunts any building, is at the town of Bishop's Waltham in this county, where many sand-martins nestle and breed in the scaffold-holes of the back-wall of WILLIAM OF WICKHAM's stables; but then this wall stands in a very sequestered and retired enclosure, and faces upon a large and beautiful lake. And indeed this species seems so to delight in large waters, that no instance occurs of their abounding but near vast pools or rivers; and in particular, it has been remarked, that they swarm on the banks of the Thames, in some places below bridge. It is curious to observe with what different degrees of architectonic skill Providence has endowed birds of the same *genus*, and so nearly correspondent in their general mode of life! For while the swallow and the house-martin discover the greatest address in raising and securely fixing crusts or shells of loam as *cunabula* for their young, the bank-martin terebrates a round and regular hole in the sand or earth, which is serpentine, horizontal, and about two feet deep. At the inner end of this burrow does this bird deposit, in a good degree of safety, her rude nest, consisting of fine grasses and feathers, usually goose feathers, very inartificially laid together. Perseverance will accomplish any thing; though one would at first be disinclined to believe, that this weak bird, with her soft and tender bill and claws, should ever be able to bore the stubborn sand-bank without entirely disabling herself. Yet with these feeble instruments have I seen a pair of

them make good dispatch, and could remark how much they had scooped that day, by the fresh sand which ran down the bank, and was of a different colour from that which lay loose and bleached in the Sun. In what space of time these little artists are able to mine and finish these cavities I have never been able to discover, for reasons given above; but it would be a matter worthy of observation, where it falls in the way of any naturalist to make his remarks. This I have often taken notice of, that several holes, of different depths, are left unfinished at the end of summer. To imagine that these beginnings were intentionally made in order to be in greater forwardness for next spring, is allowing, perhaps, too much foresight and *rerum prudentia* to a simple bird. May not the cause of these *latebræ* being left unfinished arise from their meeting in those places with *strata* too hard, harsh, and solid for their purpose, which they relinquish, and go to a fresh spot that works more freely? Or may they not in other places fall in with a soil as much too loose and mouldering, liable to flounder, and threatening to overwhelm them and their labours?

One thing is remarkable, that after some years the old holes are forsaken, and new ones bored; perhaps because the old habitations grow foul and fetid from long use; or because they may so abound with fleas as to become untenable. This species of swallow, moreover, is strangely annoyed with fleas; and we have seen fleas, bed-fleas (*pulex irritans*) swarming at the mouths of these holes like bees on the stools of their hives. When they

they happen to breed near hedges and enclosures, they are dispossessed of their breeding holes by the house-sparrow, which is on the same account a fell adversary to house-martins. The following circumstance should by no means be omitted, that these birds do *not* make use of these caverns by way of *hybernacula*, as might be expected; since banks so perforated have been dug out with care in the winter, when nothing was found but empty nests. The sand-martin arrives much about the same time with the swallow, and lays, as she does, from four to six white eggs. But as this species is *cryptogame*, carrying on the business of nidification, incubation, and the support of its young, in the dark, it would not be so easy to ascertain the time of breeding, were it not for the coming forth of the broods, which appear much about the time, or rather somewhat earlier than those of the swallow. The nestlings are supported in common like those of the *congeners*, with gnats and other small insects; and sometimes they are fed with *libellulae* (dragon-flies) almost as long as themselves. The last week in June, we have seen a row of these sitting on a rail near a great pool, as perchers, and so young and helpless, as easily to be taken by hand; but whether the dams ever feed them on the wing, as swallows and house-martins do, we have never yet been able to determine; nor do we know whether they pursue and attack birds of prey. These *hirundines* are no songsters, but rather mute, making only a little harsh noise when a person approaches their nests. They seem not to be of a sociable turn.



turn, never with us congregating with their *congeners* in the autumn. Undoubtedly they breed a second time, like the house-martin and swallow, and withdraw about Michaelmas. Though in some particular districts they may happen to abound, yet in the whole, in the South of England at least, is this much the rarest species. For there are few towns or large villages but what abound with house-martins; few churches, towers, or steeples, but what are haunted by some swifts; scarce a hamlet or single cottage-chimney that has not its swallow: while the bank-martins, scattered here and there, live a sequestered life among some abrupt sand-hills, and in the banks of some few rivers. These birds have a peculiar manner of flying; flitting about with odd jerks and vacillations, not unlike the motions of a butterfly. Doubtless the flight of all *hirundines* is influenced by, and adapted to, the peculiar sort of insects which furnish their food. Hence it would be worth enquiry to examine, what particular *genus* of insects affords the principal food of each respective species of swallow. Sand-martins differ from their *congeners* in the diminutiveness of their size and in their colour, which is what is usually called a mouse-colour. Near Valentia in Spain they are taken and sold in the markets for the table; and are called by the country people, probably from their desultory, jerking manner of flight, *papillon de Montagna*.

I am, with the greatest respect, &c.





Fig. A.

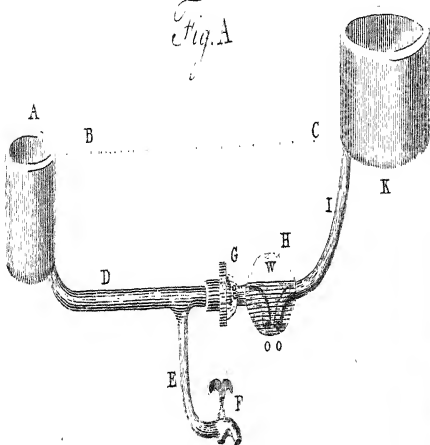


Fig. 1.

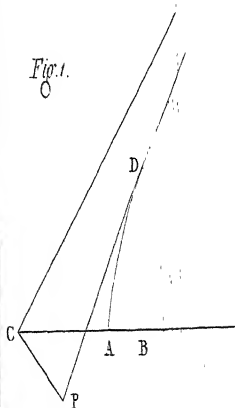


Fig. 2.

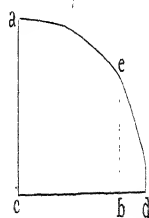


Fig. B.

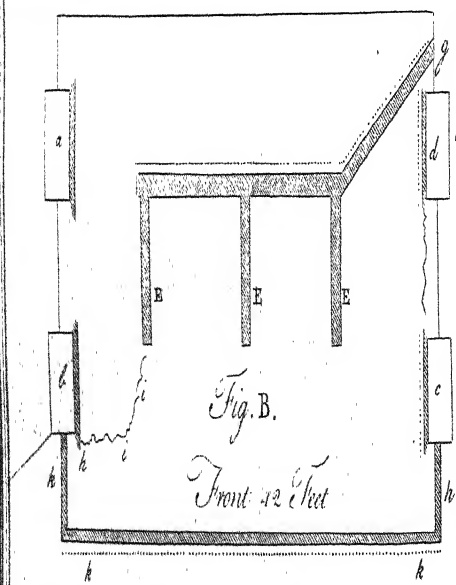


Fig. 3.

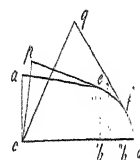
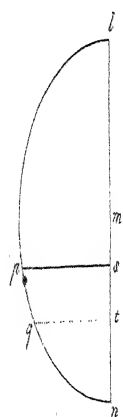


Fig. 4.





XXIV. *Account of a Machine for raising Water, executed at Oulton, in Cheshire, in 1772. In a Letter from Mr. John Whitehurst to Dr. Franklin.*

DEAR SIR,

Redde, Mar. 16, 1775. **P**RESUMING the mode of raising water by its *momentum* may be new and useful to many individuals, induces me to send you the inclosed plan and description of a work, executed in the year 1772, at Oulton, in Cheshire, the feat of PHILIP EGERTON, Esq. for the service of a brewhouse and other offices, and is found to answer effectually. I am, SIR,

Your most obedient servant,

JOHN WHITEHURST.

Please to observe, that the circumstances attending this water-work, require a particular attention, and are as follows (see TAB. VII. fig. A.): (A) represents the spring or original reservoir, whose upper surface coincides with the horizontal line-BC, and the bottom of the reservoir K. D the main pipe;  $1\frac{1}{2}$  inch diameter, and nearly two hundred yards in length. E a branch pipe, of the former dimensions,

mensions, for the service of the kitchen offices. Now it is to be observed, that the kitchen offices are situated at least eighteen or twenty feet below the surface of the reservoir A, and that the cock F is about sixteen feet below it. G represents a valve-box, *g* the valve, H an air-vessel, *oo* the ends of the main-pipe inserted into H, and bending downwards, to prevent the air from being driven out when the water is forced into it, *w* the surface of the water. Now it is well known, that water discharged from an aperture, under a pressure of sixteen feet perpendicular height, moves at the rate of thirty-two feet in a second of time; therefore such will be the velocity of the water from the cock F. And although the aperture of the cock F is not equal to the diameter of the pipe D, yet the velocity of the water contained in it will be very considerable: consequently, when a column of water, two hundred yards in length, is thus put into motion, and suddenly stopped by the cock F, its momentous force will open the valve *g*, and condense the air in H, as often as water is drawn from F. In what degree the air is thus condensed, is needless to say in the instance before us; therefore I shall only observe, that it was sufficiently condensed to force out the water into the reservoir K, and even to burst the vessel H, in a few months after it was first constructed, though apparently very firm, being made of sheet lead, about nine or ten pounds weight to a square foot. From whence it seems reasonable to infer, that the momentous force is much

superior

superior to the simple pressure of the column IK; and therefore equal to a greater resistance (if required) than a pressure of four or five feet, perpendicular height. It seems necessary further to observe, that the consumption of water in the kitchen offices is very considerable; that is, that water is frequently drawing from morning till night all the days of the year.



XXV. *Extract of a Letter from Mr. Lexel to Dr. Morton.*  
*Dated Petersburg, June 14, 1774.*

Redde, Mar. 16,  
 1775.

AS I propose to make some researches concerning the difference of the meridians of the principal Observatories of Europe, which I am persuaded can best be ascertained by the occultations of the fixed stars by the Moon; it would be of great service to me to be furnished with the observations that have been made, or that will be made, this year, of the occultations of  $\alpha$  or of  $\gamma$  Tauri by the Moon. I beg, therefore, SIR, you will please to desire Mr. MASKELYNE to communicate them to me, towards the beginning of the next year, directed to Mr. EULER, secretary of our Academy. It would also be of great use to me to have the observation of the occultation of the Pleiades by the Moon the 15th of March, 1766, in case it has been taken at Greenwich.

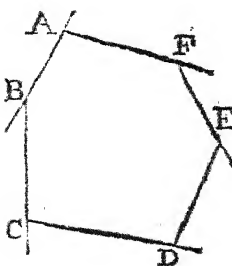
Here are some observations of Mr. Wargentín, of the occultations of  $\alpha$  and  $\gamma$  Tauri.

1773, Nov.	11	56	12	Emerſion of $\alpha$ , uncertain to ſome ſeconds.
1774, Jan.	22	6	0 26 $\frac{1}{2}$	Immersion of the eye of $\gamma$ , } both very certain,
		7	15 51	Emerſion,
Feb.	18	6	39 51	Immersion of $\gamma$ , very certain.
		7	19 53	Emerſion, within two ſeconds.

The following are my obſervations.

1773, Nov.	11	56	47	{ Emerſion of $\alpha$ almoſt certain; the immersion was not obſerved on account of clouds.
1774, Jan.	22	7	2 52	Immersion, }
		8	20 44	Emerſion, }
				} both certain.
April	14	8	28 34	Immersion of $\alpha$ , very certain.
		9	3 20	Emerſion of the ſame.
	15	9	32 0	Immersion of FLAMSTEAD'S 115 in $\gamma$ .
	16	10	21 31	Immersion of a ſtar of the 6th magnitude in $\pi$ .
May	22	13	2 20	Immersion of $\pi$ Virginis, very certain.

I have lately diſcovered two curious theorems, which I ſhall here communicate to the Royal Society.



# T H E O R E M.

Let A, B, C, D, E, F, be a polygon whoſe ſides are named  $a, b, c, d, e, f$ ; and the exterior angles  $\alpha, \beta, \gamma, \delta, \epsilon, \zeta$ , ſo that the ſide  $a$  be placed between the angles  $\alpha$  and  $\beta$ ,  $b$  between  $\beta, \gamma$ , &c.

$$1. \frac{a \times \sin. \alpha + b \times \sin. (\alpha + \beta) + c \times \sin. (\alpha + \beta + \gamma) + d \times \sin. (\alpha + \beta + \gamma + \delta) + e \times \sin. (\alpha + \beta + \gamma + \delta + \epsilon) + f \times \sin. (\alpha + \beta + \gamma + \delta + \epsilon + \zeta)}{2} = s.$$

$$2. \frac{a \times \operatorname{cofin.} z + b \times \operatorname{cof.} (z + \beta) + c \times \operatorname{cof.} (z + \beta + \gamma) + d \times \operatorname{cof.} (z + \beta + \gamma + \delta) + e \times \operatorname{cof.} (z + \beta + \gamma + \delta + \epsilon) + f \times \operatorname{cof.} (z + \beta + \gamma + \delta + \epsilon + \zeta)}{2} = 0.$$

In fact it is  $\operatorname{fin.} (a + \beta + \gamma + \delta + \epsilon + \zeta) = \operatorname{fin.} 360^\circ = 0$ , and  $\operatorname{cof.} (a + \beta + \gamma + \delta + \epsilon + \zeta) = +1$ .; but in order to give the same form to the two expressions, I rather chose to represent them as I have done. By means of these two theorems the solution of polygons will be as easy as that of triangles by common trigonometry.

XXVI. *An Investigation of a general Theorem for finding the Length of any Arc of any Conic Hyperbola, by Means of Two Elliptic Arcs, with some other new and useful Theorems deduced therefrom.* By John Landen, F.R.S.

Redde, Mar. 23, 1775. **I**N a paper, which the Society did me the honour to publish in the Philosophical Transactions for the year 1771, I announced, that I had discovered a general theorem for finding the length of any arc of any conic hyperbola, by means of two elliptic arcs; and I promised to communicate the investigation of such theorem. I now purpose to perform my promise; and, being pleased with the discovery (by which we are enabled to bring out very elegant conclusions in many interesting enquiries, as well mechanical as purely geometrical), I cannot but flatter myself, that what I am about to communicate will be acceptable to gentlemen who are curious in such inquiries.

I. From the theorem taken notice of in Art. I. of the paper I have just now mentioned, it follows, that in the hyperbola AD (TAB. VII. fig. 1.), if the semi-transverse axis  $AC = m - n$ ; the semi-conjugate  $= 2 \times \sqrt{mn}$ ; and the perpendicular CP, from the center c upon the tangent DP,  $= \sqrt{m^2 - n^2} - \frac{2n^2}{m+n}$ ; the difference (DP - AD) between the said tangent

tangent  $DP$  and the arc  $AD$  will be equal to the fluent of

$$\frac{\sqrt{m^2 - t^2 - t^2}}{m + n - t^2} \times t.$$

2. It is well known, that in any ellipsis whose femi-transverse axis is  $m$ , and femi-conjugate  $n$ ; if  $x$  be the abscissa, measured from the center upon the transverse axis, and  $z$  the arc between the conjugate axis and the ordinate corresponding to  $x$ ,  $\frac{\sqrt{m^2 - x^2}}{n^2 - x^2} \times x$  will be  $= z$ ,

$$g \text{ being } = \frac{m^2 - n^2}{m^2}.$$

$$\text{Hence, } \frac{\sqrt{m^2 - t^2 - t^2}}{m - n - t^2} \times t \text{ being } = \frac{\sqrt{m^2 + n^2 - t^2}}{m + n - \frac{m+n}{m-n} t^2} \times \frac{m+n}{m-n} t, \text{ it}$$

appears, that in the ellipsis  $aed$  (fig. 2.) whose femi-transverse axis  $cd$  is  $= m + n$ , femi-conjugate  $ca = 2 \times mn^{\frac{1}{2}}$ , and abscissa  $cb$  (corresponding to the ordinate  $be$ )

$$= \frac{m+n}{m-n} t; \text{ the arc } ae \text{ is equal to the fluent of } \frac{\sqrt{m^2 + n^2 - t^2}}{m - n - t^2} \times t.$$

3. In the ellipsis  $aefd$  (fig. 3.), the femi-transverse axis  $cd$  being  $= m$ ; the femi-conjugate  $ca = n$ ; and the abscissa  $cb$  (corresponding to the ordinate  $be$ )  $= x$ ; if  $ep$ , the tangent at  $e$ , intercepted by a perpendicular ( $cp$ ) drawn thereto from the center  $c$ , be denoted by  $t$ ;  $gx \times \frac{\sqrt{m^2 - x^2}}{m^2 - gx^2}$  (as is well known) will be  $= t$ ,  $g$  being as in the preceding article.

Hence

Hence  $x^2 = \frac{m^2 g + t^2}{2g} - \frac{\overline{m^2 - n^2}^2 - 2 \times m^2 + n^2 \times t^2 + t^4}{2g}^{\frac{1}{2}}$ . From

which equation, by taking the fluxions, we have,

$$\begin{aligned} x \dot{x} &= \frac{t \dot{t}}{2g} + \frac{\overline{m^2 + n^2} \times t \dot{t} - t^3 \dot{t}}{2g \times \overline{m^2 - n^2}^2 - 2 \times m^2 + n^2 \times t^2 + t^4}^{\frac{1}{2}} \\ &= \frac{t \dot{t}}{2g} + \frac{\overline{m^2 + n^2} \times t \dot{t} - t^3 \dot{t}}{2g \times \overline{m^2 - n^2}^2 - t^2 \times m^2 + n^2 - t^2}^{\frac{1}{2}}. \end{aligned}$$

But  $\dot{x}$  being =  $\frac{\overline{m^2 - g x^2}}{m^2 - x^2}^{\frac{1}{2}} \times \dot{x}$ ,

as observed in the preceding article, it appears that  $\frac{g}{t} \times x \dot{x}$  is =  $\dot{x}$ . It is obvious, therefore, that

$$\begin{aligned} \dot{x} \text{ is } &= \frac{1}{2} \dot{t} + \frac{1}{2} \times \frac{\overline{m^2 + n^2} \times t \dot{t} - t^3 \dot{t}}{\overline{m^2 - n^2}^2 - t^2 \times m^2 + n^2 - t^2}^{\frac{1}{2}} \\ &= \frac{1}{2} \dot{t} + \frac{1}{4} \times \frac{\overline{m-n}^2 \times t \dot{t} - t^3 \dot{t}}{\overline{m-n}^2 - t^2 \times m + n^2 - t^2}^{\frac{1}{2}} + \frac{1}{4} \times \frac{\overline{m+n}^2 \times t \dot{t} - t^3 \dot{t}}{\overline{m-n}^2 - t^2 \times m + n^2 - t^2}^{\frac{1}{2}} \\ &= \frac{1}{2} \dot{t} + \frac{1}{4} \times \frac{\overline{m-n}^2 - t^2}{\overline{m+n}^2 - t^2}^{\frac{1}{2}} \times \dot{t} + \frac{1}{4} \times \frac{\overline{m+n}^2 - t^2}{\overline{m-n}^2 - t^2}^{\frac{1}{2}} \times \dot{t}. \end{aligned}$$

taking the fluents by the theorems in art. 1. and 2. we

have  $x = ae$  (fig. 3.) =  $\frac{1}{2} t + \frac{DP - AD}{4}$  (fig. 1.) +  $\frac{ae}{4}$  (fig. 2.)

consequently the hyperbolic arc AD is = DP + ae + 2 t - 4 ae.

Thus, beyond my expectation, I find, that the hyperbola may in general be rectified by means of two ellipses.

Writing E and F for the quadrantal arcs ad, ad, (fig. 2. and 3.) respectively, and L for the limit of the difference DP - AD, whilst the point of contact (p) is supposed to be carried to an infinite distance from the vertex A of the hyperbola (fig. 1.), we find

$$2F - E = L,$$

the value of ae being =  $\frac{1}{2} F + \frac{1}{2} EL - \frac{1}{2} E$  when  $t$  is =  $m - n$ ;

that

that is, when e coincides with d (fig. 2.), and p with c (fig. 1.), by what I have proved in the before mentioned paper, art. 10.

4. From what is done above, the following useful theorems are deduced.

THEOREM I.

The fluent of  $\frac{1}{2}a^{\frac{1}{2}}z^{-\frac{1}{2}}z \times \frac{\sqrt{\frac{b^2}{a^2}+z}}{a-z}$  is = de.

THEOREM II.

The fluent of  $\frac{1}{2}a^{\frac{1}{2}}z^{-\frac{1}{2}}z \times \frac{\sqrt{\frac{a^2}{b^2}-z}}{\frac{b^2}{a}+z} = \frac{2a^2}{b^2+1} \cdot \text{de} - \frac{2a^2}{b^2+2} \cdot \text{ef}.$

THEOREM III.

The fluent of  $\frac{\frac{1}{2}a^{\frac{1}{2}}z^{\frac{3}{2}}z}{b^2+2kz-z^2} = 2 \text{ef} - \text{de} = 2 \text{F} - \text{E} + \text{AD} - \text{DP}.$

THEOREM IV.

The fluent of  $\frac{\frac{1}{2}a^{-\frac{1}{2}}b^2z^{-\frac{1}{2}}z}{b^2+2kz-z^2} = 2 \times \text{de} - \text{ef}.$  N.B.  $k = \frac{a^2-b^2}{2a}.$

These theorems still refer to fig. 1. 2. 3.; but now the values of the several lines therein (being not as before) are as here specified; *videlicet*,

Fig. 1. In the hyperbola AD, the semi-transverse axis AC is now =  $a$ ; the semi-conjugate =  $b$ ; the perpendicular CP, from the center c upon the tangent DP, is =  $a^2z^{\frac{1}{2}}$  the said tangent DP =  $\frac{a}{z} \times \sqrt{b^2+2kz-z^2}$ ; and the abscissa CB (corresponding to the ordinate BD) is =  $\frac{a^2}{z^{\frac{1}{2}}} \times \frac{\sqrt{az+b^2}}{a^2+b^2}$

Fig.

Fig. 2. In the ellipsis aed, the semi-transverse axis cd is  $= \sqrt{a^2 + b^2}^{\frac{1}{2}}$ ; the semi-conjugate ca = b; the abscissa cb =  $\frac{a^2 + b^2}{a} \times \sqrt{a - z}^{\frac{1}{2}}$ ; and the ordinate be =  $b \times \sqrt{\frac{z}{a}}^{\frac{1}{2}}$ .

Fig. 3. In the ellipsis aefd, the semi-transverse axis cd is  $= \frac{1}{2} \sqrt{a^2 + b^2}^{\frac{1}{2}} + \frac{1}{2} a$ ; the semi-conjugate ca =  $\frac{1}{2} \sqrt{a^2 + b^2}^{\frac{1}{2}} - \frac{1}{2} a$ ; the tangents ep, fq, intercepted by perpendiculars (cp, cq) drawn thereto from the center c, each =  $a^{\frac{1}{2}} \times \sqrt{a - z}^{\frac{1}{2}}$ ; and the abscissa (cb' or cb'') on cd, corresponding to the point e or f, of the curve is determined by the expression

$$\frac{\sqrt{a^2 + b^2}^{\frac{1}{2}} + a - z \mp z^2 + \frac{b^2}{a} z^{\frac{1}{2}}}{2^{\frac{1}{2}} + \sqrt{a^2 - b^2}^{\frac{1}{2}}} \times cd.$$

The quadrantal arc ad (fig. 2.) is denoted by E; and the quadrantal arc ad (fig. 3.) is denoted by F. L the limit of DP - AD (fig. 1.) is = 2 F - E.

From what is now done, I might proceed to deduce many other new theorems, for the computation of fluents; but I shall, at present, decline that business: and, after giving a remarkable example of the use of theorem 4. in computing the descent of a heavy body in a circular arc, conclude this paper with a few observations relative to the contents of the preceding articles.

5. Let lpqn (fig. 4.) be a semi-circle perpendicular to the horizon, whose highest point is l, lowest n, and center m. Let ps, qt, parallel to the horizon, meet the diameter lmn in s and t; and let the radius lm (or mn) be denoted by r; the height ns by d; and the distance



st by  $x$ . Then, putting  $b$  for  $(16\frac{1}{12}$  feet) the space a heavy body, descending freely from rest, falls through in one second of time; and supposing a pendulum, or other heavy body, descending by its gravity from  $p$ , along the arc  $pqn$ , to have arrived at  $q$ ; the fluxion of the time

of descent will be  $= \frac{\frac{1}{2}rb^{-\frac{1}{2}}x^{-\frac{1}{2}}}{2rd-d^2-2.r-d.x-x^{\frac{1}{2}}}$ . The fluent

whereof, or the time of descent from  $p$  to  $q$  is (by theor. 4. of the preceding article)  $= \frac{2r}{b^{\frac{1}{2}} \times 2r-d} \times de-ef$ .

$a$  (in that theorem) being taken  $=d^{\frac{1}{2}}$ ,  $b=2r-d^{\frac{1}{2}}$ ,  $cb$  (fig. 2.)  $=\frac{2r}{d^{\frac{1}{2}}} \times \overline{d-x^{\frac{1}{2}}}$ , and  $cp$ ,  $fq$ , (fig. 3.) each  $=\overline{d-x^{\frac{1}{2}}}^{\frac{1}{2}}$ .

Hence it appears, that the whole time of descent from  $p$  to  $n$  is  $= \frac{2r}{b^{\frac{1}{2}} \times 2r-d} \times \overline{E-F}$ ; when, in fig. 2. and 3. the semi-axes are taken according to the values of  $a$  and  $b$  just now specified.

6. If  $pqn$  be a quadrant; that is, if  $d$  be  $=r$ , the whole time of descent from  $p$  to  $n$  will be  $= \frac{2}{b^{\frac{1}{2}}} \times \overline{E-F}$ , by the above theorem. Which time, by what I have shewn in the Philof. Tranfact. for 1771, is  $= \frac{1}{b^{\frac{1}{2}}} \times \frac{1}{2}E + \frac{1}{2}\sqrt{E^2-2c}$ ,  $c$  being  $\frac{1}{4}$  of the periphery of the circle whose radius is  $r$ .

Consequently,  $\frac{2}{b^{\frac{1}{2}}} \times \overline{E-F}$  being found  $= \frac{1}{b^{\frac{1}{2}}} \times \frac{1}{2}E + \frac{1}{2}\sqrt{E^2-2c}$ ,

we find from that equation  $F = \frac{3}{4}E - \frac{1}{4}\sqrt{E^2-2c}$ , where  $E$  is the quadrantal arc of the ellipsis, whose semi-transverse and semi-conjugate axes are  $2r^{\frac{1}{2}}$  and  $r^{\frac{1}{2}}$ ; and  $F$  the qua-

dramtal arc of another ellipsis, whose semi-transverse and semi-conjugate axes are  $\frac{\sqrt{a^2 + b^2}}{2} + \frac{1}{2}r^{\frac{1}{2}}$  and  $\frac{\sqrt{a^2 + b^2}}{2} - \frac{1}{2}r^{\frac{1}{2}}$ .

Before Mr. MACLAURIN published his excellent Treatise of Fluxions, some very eminent mathematicians imagined, that the *elastic curve* could not be constructed by the quadrature or rectification of the conic sections. But that gentleman has shewn, in that treatise, that the said curve may in every case be constructed by the rectification of the hyperbola and ellipsis; and he has observed, that, by the same means, we may construct the curve along which, if a heavy body moved, it would recede equally in equal times from a given point. Which last mentioned curve Mr. JAMES BERNOULLI constructed by the rectification of the elastic curve, and Mr. LEIBNITZ and Mr. JOHN BERNOULLI by the rectification of a geometrical curve of a higher kind than the conic sections. It is observable, that Mr. MACLAURIN's method of construction just now adverted to, though very elegant, is not without a defect. The difference between the hyperbolic arc and its tangent being necessary to be taken, the method always fails when some principal point in the figure is to be determined; the said arc and its tangent then both becoming infinite, though their difference be at the same time finite. The contents of this paper, properly applied, will evince, that both the *elastic curve* and the *curve of equable recess from a given point* (with many others) may be constructed by the rectification of the ellipsis only, without failure in any point

XXVII. *Observations made at Chislehurst, in Kent, in the Year 1774. By the Rev. Francis Wollaston, LL.B. F.R.S.*

Redde, Mar. 16, 1775. **H**AVING now compleated my original design, and kept my clock going for a third year, without the least touch of the oil, or any alteration whatsoever, I presume the result of my observations to ascertain the rate of its going, may not be an unacceptable addition to the former papers on that subject, delivered to this Society. The regular difference between the summer and winter months, and some degree of similarity between those differences, seems to shew a regularity in the cause. What that may be, is not fully to be ascertained hereby; though it seems to have been difference of moisture, rather than of heat. By comparing these three last years with that which I first gave, when the clock was in some degree foul, it seems as if it were most affected when the work is clean. Yet is not that quite certain; for the differences, which decreasing gradually in the following table, would justify this conclusion, it may be observed, increase again in the last instance.

			per Day. Diff.
The first greatest loss was	Dec. 1770 or	— 1,9	"
The next greatest gain	June 1771	+ 2,4	4,3
The next greatest loss	Oct. 1771	— 1,9	4,3

The clock was cleaned November 1771, and might not be reckoned to get to any stated rate till the beginning of the next year; after which,

			Diff.
The first greatest loss was	Feb. 1772 or	— 0,86	
The next greatest gain	July 1772	+ 7,83	8,69.
The next greatest loss	Jan. 1773	— 0,09	7,92
The next greatest gain	Aug. 1773	+ 6,17	6,26
The next least gain	Feb. 1774	+ 0,30	5,87
The next greatest gain	Aug. 1774	+ 4,95	4,65
The next greatest loss to the end of that year,	} Dec. 1774	— 0,90	5,85

Hereby July and August appear to be the months for greatest acceleration, and January and February for retardation; contrary to the affection of metalline rods, but agreeable to the effect to be expected from moisture upon wood. Yet this difference is not so great in any degree, nor (what is more material to observation) by any means so sudden in its changes, as what is occasioned by heat upon metals. And even this, perhaps, might be obviated by a strong coat of varnish on the rod, or some preparation of the wood itself. One thing it may be proper to mention, as an accidental experience I have had the last year; that a clock so fixed, with

with a pendulum of so simple construction, is not easily affected by any tremulous motion of the building to which it is fastened. In the months of March, April, and part of May, I had occasion to make alterations in the top of my house, in order to gain more rooms in it; and notwithstanding the great jarring necessarily consequent upon taking off the old rafters, and laying on a new leaded roof, and new joists and floor over the observatory itself, the clock seems not to have been disordered at all by it. Between February 7th and 20th

there will appear an error in the calculation of gain, to any one who shall take the trouble to examine it: not that I believe there is really any error in it; but by an accident in winding the clock (not having put down the spring sufficiently, which is intended to keep on the motion of the wheels,) there were 6" lost, as appeared by the assistant clock, which had been set with it just before. These 6" being allowed for, will reduce the loss of

" 2,1 to a gain of 3,9 in that interval of thirteen days,

— " 2,1 + " 6 = + 3,9. In the months of February, March, and April, I was frequently from home; so that the state of the thermometer and barometer, if I were to set them down, would be very imperfect. In the other months they are more complete; yet there may, perhaps, in my absence, have been some days in them either higher or lower than what are here given.

		Clock + too f. ft. — too slow. for mean fol. time.	Numb. of Days.	Gain + or Loss —	Rate per Day.	Throwing out	
						South side.	North side.
1773.							
Dec.	27	Clock + 32 19,7					
			7	+	7,4 + 1,03		
1774.							
Jan.	3	+ 32 26,9		+	14,8 + 0,42	I 25	I 23
Feb.	7	+ 32 41,7	55	+			
	13	Loss in winding 6"	13	+	3,9 + 0,30	I 27	I 29
	20	+ 32 32,6	20	+	6,0 + 0,30	I 35	I 38
Mar.	12	+ 32 45,6	8	+	2,4 + 0,30	I 28	I 31
	20	+ 32 48,0	13	+	5,1 + 0,39	I 40	I 43
April	2	+ 32 53,1	29	+	52,0 + 1,79	I 35	I 40
May	1	+ 33 45,1	25	+	70,1 + 2,80	I 36	I 40
	26	+ 34 55,2	13	+	45,4 + 3,49	I 40	I 43
June	8	+ 35 40,6	14	+	49,9 + 3,57	I 38	I 42
	22	+ 36 30,5	9	+	37,3 + 4,14	I 36	I 40
July	1	+ 37 7,8	31	+	134,7 + 4,34	I 37	I 40
August	1	+ 39 22,5	18	+	86,9 + 4,83		
	19	+ 40 49,4	15	+	74,6 + 4,95	I 32	I 35
Sept.	3	+ 42 3,7	9	+	33,3 + 4,70	I 35	I 38
	12	+ 42 37,0	21	+	68,3 + 3,25	I 35	I 38
Oct.	3	+ 43 45,3	12	+	23,9 + 1,99	I 33	I 36
	15	+ 44 9,2	14	+	28,7 + 2,05	I 30	I 33
	29	+ 44 37,9	14	+	29,8 + 2,11	I 30	I 33
Nov.	12	+ 45 7,7	23	+	19,4 + 0,84		
Dec.	5	+ 45 27,1	8	—	1,7 — 0,21		
	13	+ 45 25,4	11	—	6,9 — 0,63		
	24	+ 45 18,5	8	—	7,2 — 0,90	I 20	I 23
1775.							
Jan.	1	+ 45 11,3					

		Thermometer without doors exposed to the North.			Therm. near the clock.	Barometer on the ground floor.	Hygrom. near the clock.
		Hor. 8 A. M.	Hor. 2. P. M.	Hor. 11. P. M.	Hor. 9. A. M.		
1771.							
Jan.	{ Highest	48	50	48	47	29,85	70
	{ Lowest	23,5	29	23	31	28,67	54
May	{ Highest	60	67	53	57	29,98	23
	{ Lowest	45	45,5	40	47	29,10	10
June	{ Highest	70	74	60	65	30,11	20
	{ Lowest	51	55	47	51	29,24	9
July	{ Highest	72	82	60	65	30,14	19
	{ Lowest	57	57	50	53	29,52	7
August	{ Highest	70	76	63	65	30,12	34
	{ Lowest	57	61	50	53	29,32	5
Sept.	{ Highest	70	72	60	63	30,06	32
	{ Lowest	49	51	46	51	29,915	11
Oct.	{ Highest	56	64	54	55	30,37	27
	{ Lowest	39	45	39	44	29,23	13
Nov.	{ Highest	54	59	51	49	30,06	42
	{ Lowest	31,5	33	31	34	29,02	20
Dec.	{ Highest	50	53	51	45	30,54	70
	{ Lowest	21	30,5	25	30	28,95	27

Accidents of weather and various avocations have prevented me from making any other observations in the course of the last year; excepting the second disappearance of Saturn's ring, and re-appearance of it again, both of which I was fortunate enough to observe. I had seen the ring many times after its first re-appearance; and observed it to be lessening again, till it was become but  
a mere

a mere thread of light, Monday, April 4, though certainly visible then. Tuesday, April 5, the evening was very clear, yet no ring could I perceive with my  $3\frac{1}{2}$  feet achromatic telescope; nor from that time did I see any thing of it (but, during part of the interval, the appearance of a dark belt across the planet,) till Thursday, June 30, when I thought I saw the preceding *ansa*. Saturday, July 2, I am sure I saw the whole ring again, as a thread of light; and as the preceding *ansa*, or end of that thread, appeared larger than the subsequent one, it probably was visible, and not only a deception, when I fancied I perceived it before. In these observations it deserves to be remarked, that the magnifying power of 100 seemed, from its brightness, to shew the thread of the ring more visibly than 150.

Chislehurst,  
Jan. 1775.

FRANCIS WOLLASTON.



**XXVIII. Of Triangles described in Circles and about them.**  
*By John Stedman, M.D.*

Redde, March 23, 1775.

**PROPOSITION I.**  
*An equilateral triangle inscribed within a circle is larger than any other triangle that can be inscribed within the same circle.*

LET ABC (TAB. VIII. fig. 1.) be an equilateral triangle, inscribed in the circle ADCB; and let ADE be a triangle supposed larger than ABC. Let ADE be drawn with one of its angles at the same point with one of the angles of the equilateral triangle, suppose at A, and then its other two angles will fall either on the segments ADB and AEC, or one of the angles will on the segment BC. First, let one of its angles fall at D, between A and B; and the other at E, between A and C, and draw the line BE. In the triangles ABC, ABE, the triangle ABF is common, and the two remaining triangles BFC, AFE, are similar; for the angle AFE is equal to its opposite angle BFC; and the two angles FAC, EBC, are equal, being subtended by the same segment FC, and so the two remaining angles AEF,

AEF, BCF, must be equal; wherefore the sides are proportional, and BC and AE, subtending equal angles must be homologous; but BC is equal to AC, which is greater than AE; consequently the triangle BFC is greater than AFE, and so the equilateral triangle ABC is greater than the triangle ABE. In the same manner, the triangle ABE may be proved greater than ADE; for ABE is common, and the two triangles ADH, BHE, are similar, and their sides proportional; and AD and BE, subtending equal angles, must be homologous; but BE is greater than BC, which is equal to AB, and that again greater than AD; consequently BE is greater than AD, and the whole triangle AEB greater than AED; and so the equilateral triangle must, *à fortiori*, be greater than AED.

### 2. E. D.

Next, let the triangle ABE be supposed greater than the equilateral triangle ABC, and let the angle ADE fall somewhere in the segment BDC, (see fig. 2.) so that the segment EC may be greater than BD; for if it were not, the angle AED being applied to any of the angles of the equilateral triangle, the demonstration would become the same as in the first case: wherefore the segments AEC, BDC, being equal, and BD being less than EC, AE must be less than DC. Draw the right line DC, and then in the two triangles ADC, ADE, the triangle AFD is common, and the two triangles AFE, DFC, are equiangular and similar, and the sides AE, DC, subtending equal angles, are homologous; but DC is greater than AE; so the triangle DFC is greater than the triangle AFE, and the whole

triangle  $ADC$  is greater than  $ADE$ ; but the equilateral triangle may be proved greater than  $ADC$  from the first case, and consequently greater than  $ADE$ . *Q. E. D.*

### PROPOSITION II.

*An equilateral triangle described about a circle is less than any other triangle that can be described about the same circle. Fig. 3.*

LET the equilateral triangle  $ABC$  be described about the circle  $HIX$ , and let the triangle  $BDG$  be supposed less than the equilateral triangle. Draw the line  $AF$  parallel to  $BC$ , then the triangles  $AFE$ ,  $EGC$ , are similar; for the opposite angles  $AEF$ ,  $GEC$ , are equal, as likewise the angle  $AFE$  to the angle  $EGC$ ; the lines  $AF$  and  $GC$  being parallel, and falling upon the same line  $FG$ , the angles  $AFE$  and  $EGC$  are therefore equal, and the sides  $AE$ ,  $EC$ , subtending equal angles, are homologous; but the side of the equilateral triangle  $AC$  being equally divided at  $I$ , the line  $AE$  is greater than  $EC$ , and consequently the triangle  $AFE$  is larger than the triangle  $EGC$ ; and the triangle  $DAE$  much larger than  $EGC$ : therefore, in the triangles  $DBG$  and  $ABC$  the part  $ABGE$  being common the whole triangle  $DBG$  is larger than the equilateral triangle. *Q. E. D.*

Whatever other triangles can be described about a circle, may be demonstrated to be larger than an equilateral triangle described about the same circle, upon the same principles as the preceding.

PROPOSITION III.

*The square of the side of an equilateral triangle inscribed in a circle is equal to a rectangle under the diameter of the circle, and a perpendicular let fall from any angle of the triangle upon the opposite side. Fig. 4.*

THE two triangles ADC, AEC, are equi-angular and similar, the angles ACD, AEC, being both right, and that at A common; therefore  $AD : AC :: AC : AE$ , and  $AC^2 = AD \times AE$ . Q.E.D. (1).

The square of one side of the triangle being completed so as to include the triangle, I say, that part of the side of the square that falls within the circle is equal to the radius; and the other part, lying without the circle, is equal to the radius *minus* twice the portion lying between the side of the square, and the circumference of the circle; or is equal to that part of the radius that lies between the centre and the side of the square *minus* the remainder of the radius; that is CL

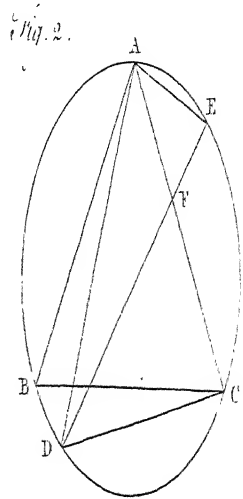
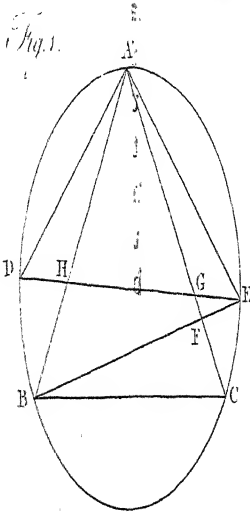
(2). And universally, a perpendicular being drawn from any angle of a right-lined triangle to the opposite side, the rectangle under the two sides which contain that angle, is equal to the rectangle under the perpendicular and the diameter of the circumscribed circle. (See TAB. VIII. fig. 5.)

From A, one angle of the triangle BAC, draw AE perpendicular to the side BC. Round the triangle BAC describe a circle, and draw the diameter AD. I say, the rectangles  $AC \times AB$ ,  $AE \times AD$ , are equal. Join DB. The angle DBA is a right angle. Therefore it is equal to the right angle CEA. The angles at the circumference ACE, ADB, are equal, because they stand upon the same arc AB. Therefore the two triangles ACE, ADB, are similar, and  $AC : AE = AD : AB$ . Therefore  $AC \times AB = AE \times AD$ . Q. E. D. S. HORSLEY.

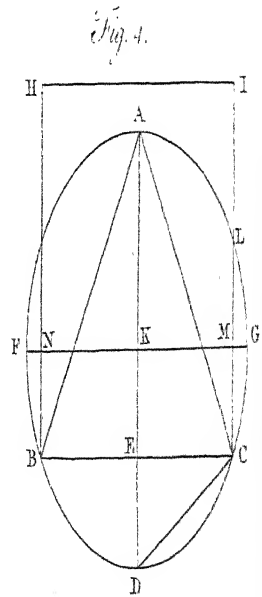
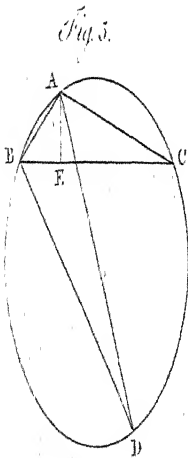
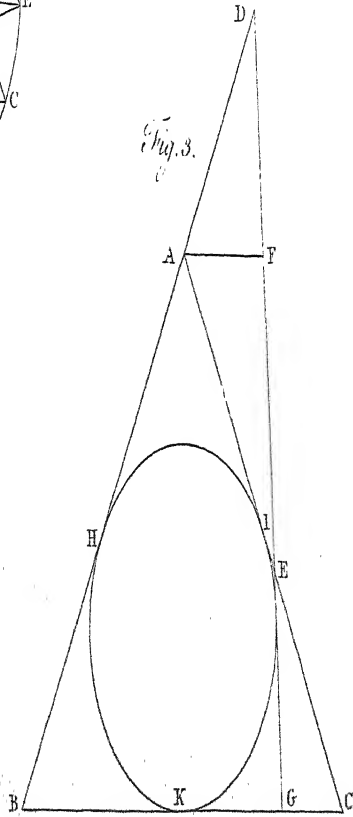
is equal to the radius, and  $LI = KG - 2MG$ ; or  $LI = KM - MG$ ,  $FG$  being parallel to  $BC$ , and consequently perpendicular to  $IC$ , must divide the chord  $LC$  in two equal parts; so that  $MC$  being equal to  $KE$ ,  $LC$  must be equal to  $2KE$ ; but  $KE$  (by EUCL. I. XIII. pr. 12. cor. 2. Clav.) is equal to  $ED$ ; therefore  $LC = KD$  the radius. The side of the square  $IC$  being equal to  $BC$  is likewise equal to  $NM$ ; but  $LC$  being equal to  $KE$ , the remaining part  $LI$  must be equal to  $NK - MG$ ; or to  $KM - MG$ . Q. E. D.







*Fig. 3.*







XXIX. De Polygonis *Area* vel Perimetro maximis et minimis, inscriptis Circulo, vel Circulum circumscribentibus.  
Auctore S. Horsley, LL. D. R. S. Sec.

Redde, May 19, 1775.

THEOREMA I. (TAB. IX.)

*Si linea recta arcum circulem contingentibus duabus interceptum contingat, segmentum ejus, contingentibus primo positis interceptum, in contactus sui puncto vel æqualiter vel inæqualiter divisum est, prout arcus ipse circularis æqualiter vel inæqualiter in eodem puncto divisus est. Segmentaque arcus (inæqualiter scilicet divisi) et rectæ contingentis majora et minora ab iisdem sunt partibus mutui contactus.*

**A**RCUM circulem AEC, contingentibus duabus, AB, CD, interceptum, recta BD in puncto E contingat. Recta vero BD contingentibus primo positis, AB, CD, in punctis B, D, occurrat. Dico rectam BD in puncto E vel æqualiter vel inæqualiter divisam, prout arcus AEC æqualiter vel inæqualiter in eodem puncto E divisus est. Primo puta arcum AC in puncto E æqualiter divisum. Dico igitur et rectam BD, in puncto E, bifariam secari. Circuli AEC centrum esto F. Jungantur FE, FB, FD, quarum

rum  $FB, FD$ , circuli peripheriæ in punctis  $G, H$ , occurrant. Rectæ  $FB, FD$  arcus  $AE, CE$  bifariam dividunt. Arcus igitur  $GE, HE$ , arcum  $AE, CE$  semiffes. Æquales igitur  $GE, EH$ , propter  $AE, EC$ , ex hypothesi, æquales. Quare anguli  $BFE, DFE$  æquales. Æquales autem anguli  $BEF, DEF$ : rectus enim uterque. In triangulis igitur  $BFE, DFE$ , quæ latus commune habent  $EF$ , duo anguli  $BEF, DFE$ , duobus  $DEF, DFE$ , singuli singulis æquales. Reliqua igitur reliquis æqualia (per El. I. 26.). Quare  $BE = ED$ .

*Q. E. D.*

Jam vero puta arcum  $AC$  inæqualiter in  $E$  divisum, et segmentorum  $AE, CE$ , majus esse  $AE$ ,  $CE$  minus. (fig. 2.) Dico rectam  $BD$  inæqualiter in puncto  $E$  divisam, segmentumque  $BE$  majus esse,  $DE$  minus. Jungantur enim ut prius  $FB, FE, FD$ , quarum  $FB, FD$  peripheriæ in punctis  $G, H$ , occurrant. Rectæ  $FB, FD$ , arcus  $AE, CE$ , bifariam dividunt. Arcus igitur  $GE, HE$ , arcum  $AE, CE$ , semiffes. Cum igitur arcus  $AE$ , arcu  $CE$  major sit, arcus  $GE$  arcu  $HE$  major erit. Angulus igitur  $BFE$  angulo  $DFE$  major. Fiat angulus  $EFK$  angulo  $DFE$  æqualis. Quoniam angulus  $KFE$  angulo  $DFE$  æqualis est, nec non angulus rectus  $KEF$ , recto  $DEF$  æqualis; in triangulis  $EFK, EFD$ , quæ latus  $EF$  commune habent, anguli duo  $KFE, KEF$ , duobus  $DFE, DEF$ , singuli singulis æquales. Reliqua igitur reliquis æqualia. Latera igitur  $EK, ED$ , æqualia. Propter angulum vero  $EKF$  angulo  $EFB$  minorem, punctum  $K$  punctis,  $B, E$ , necessario interjacet. Recta igitur  $BE$ , recta  $KE$  major. Major itaque quam  $ED$ . *Q. E. D.*

## T H E O R E M A II.

*Linea recta quæ arcum circulearem contingentibus duabus interceptum in puncto medio contingit, et contingentibus primo positis hinc inde occurrit, minima est omnium quæ, eundem arcum contingentes, contingentibus primo positis intercipiuntur.*

A R C U M circulearem BED, contingentibus duabus AB, CD, interceptum recta AC in puncto E contingat, et contingentibus primo positis AB, CD, in punctis A, C, occurrat. Punctum E arcus BED medium esto. Dico rectam AC omnium minimam, quæ, arcum BED contingentes, contingentibus AB, CD, intercipiuntur. Sumatur enim in arcu BED punctum quodlibet F, et ducatur recta GH quæ circulum in F contingat. Recta vero GH contingentibus AB, CD, in punctis G, H, occurrat. Dico rectam AC rectâ GH minorem. Si parallelæ sint contingentes AB, CD (fig. 1.) res manifesta est: parallelas enim AB, CD, recta GH oblique fecat, recta autem AC normaliter. Rectæ vero AB, CD, non sint parallelæ. (fig. 2. et 3.) Si recta AC non sit minor quam GH, aut æqualis erit, aut major. Sit primo æqualis. Arcus autem BD, inæqualiter in F divisi, segmentum majus esto BF. Rectæ igitur GH, inæqualiter in F divisæ, segmentum GF majus erit (per 1. hujus). Rectæ BA, DC, productæ occurrent; occurfus esto K: rectarum vero GH, AC, occurfus esto L. Junctâ BD, per H ducatur HM rectis BD, AC, parallela: et per G ducatur GN rectæ DK parallela, quæ rectæ AC in N occurrat.

Rectæ GF, GB, quæ circulum in punctis B, F, contingentes  
 in puncto G conveniunt, æquales sunt. Pari ratione AE,  
 AB, æquales. Recta igitur GF duabus AE, AG, simul  
 sumptis æqualis est. Rursum rectæ HF, HD, quæ circulum  
 in punctis F, D, contingentes in puncto H conveniunt,  
 æquales sunt. Pari ratione CD, CE, æquales. Recta  
 igitur CD, vel CE, duabus FH, HC, simul sumptis æqualis.  
 Tres igitur GF, FH, HC, simul sumptæ, tribus AG, AE, EC,  
 simul sumptis æquales; id est, duæ GH, HC, simul sumptæ  
 duabus AG, AC, simul sumptis. Rectæ autem GH, AC, ex  
 hypothefi, æquales. Ablatis igitur GH, AC, æqualibus, re-  
 linquuntur HC, AG, æquales. Propter rectas autem AC,  
 MH, BD, parallelas, et triangulum BKD isosceles, æquales  
 sunt CH, AM: quare AG, AM, æquales. Propter parallelas,  
 autem AL, MH, rectæ GM, GH, in punctis A, L, similiter di-  
 visæ sunt: bifariam autem GM in A: quare et GH bifariam  
 in L: et propter GN, CD, parallelas, CN bifariam in L. Cum  
 CL igitur semiffis sit rectæ CN, et CE semiffis rectæ CA, erit  
 EL rectæ AN semiffis, five AN rectæ EL dupla. Rectâ au-  
 tem GH æqualiter in L divisâ, cum ejusdem rectæ, in-  
 æqualiter in F divisæ, segmentum GF majus est (per I. hu-  
 ju-), recta GF, rectis HF et duplæ FL simul sumptis æqua-  
 lis est. Rectæ autem GF recta GB æqualis. Quare GB, five  
 duæ GM, MB, simul sumptæ, duabus, HF et duplæ FL, simul  
 sumptis, five duabus, HD et duplæ FL, simul sumptis,  
 æquales. Et ablatis hinc inde MB, HD, æqualibus, re-  
 linquetur GM æqualis duplæ FL, id est duplæ EL, id est,  
 ex prius ostensis, rectæ AN. Propter æquales autem GA,  
 AM, recta GM rectæ GA dupla est: et propter parallelas GN,

$\kappa c$ , triangula  $AKC$ ,  $AGN$  similia: latera autem  $KA$ ,  $\kappa c$  æqualia: æqualia igitur  $GA$ ,  $GN$ . Duæ igitur  $GA$ ,  $GN$ , simul sumptæ, duplæ  $GA$  æquales sunt; id est, rectæ  $GM$ . Rectæ autem  $GM$ ,  $AN$  ostensæ sunt æquales. Duæ igitur  $GA$ ,  $GN$ , simul sumptæ, rectæ  $AN$  æquales sunt: duo nempe trianguli latera simul sumpta æqualia reliquo. Quod est absurdum. Non sunt igitur  $AC$ ,  $GH$ , æquales. Dico neque majorem esse  $AC$  quam  $GH$ . Esto enim major  $AC$ , siquidem esse potest. Duæ  $GH$ ,  $HC$  simul sumptæ duabus  $AG$ ,  $AC$  simul sumptis, ut prius, æquales sunt. Cum igitur  $AC$  major sit quam  $GH$ , erit  $AG$  minor quam  $HC$ . Æquales autem  $HC$ ,  $AM$ , ut prius. Ergo  $AG$  minor erit quam  $AM$ ; ac proinde, propter rectarum  $GM$ ,  $GH$ ,  $NC$ , similem in punctis  $A$ ,  $L$ , divisionem,  $GL$  minor quam  $LH$ , et  $NL$  minor quam  $LC$ . Cum igitur  $CE$  semissis est rectæ  $CA$ , et  $CL$  major quam semissis rectæ  $CN$ , erit  $EL$  minor quam semissis rectæ  $AN$ : sive  $AN$  duplâ  $EL$  major. Porro cum rectæ  $GH$  inæqualiter in  $L$  divisæ, segmentum  $GL$ , ex ostensis, minus est, ejusdem autem rectæ inæqualiter in  $F$  divisæ segmentum  $GF$  majus, (per 1. hujus) duæ,  $FH$  et duplâ  $FL$ , simul sumptæ rectâ  $GF$  majores sunt, sive æquali  $GB$ , sive duabus  $GM$ ,  $MB$  simul sumptis. Et æqualibus  $FH$ ,  $MB$  hinc inde ablatiis, relinquetur duplâ  $FL$  rectâ  $GM$  major. Æquales autem  $LF$ ,  $LE$ . Quare duplâ  $EL$  rectâ  $GM$  major: et  $AN$ , quæ duplâ  $EL$  jam ostensa est major, rectâ  $GM$  multo major erit. Propter  $GA$  minorem vero quam  $AM$ , rectâ  $GM$  duplâ  $AG$  major. Æquales autem ut prius  $GA$ ,  $GN$ . Duplâ igitur  $GA$ , duabus  $GA$ ,  $GN$  simul sumptis æqualis est. Rectâ igitur  $GM$ ,

duabus GA, GN simul sumptis major. Et proinde recta AN, quæ ostensa est major quàm GM, duabus GA, GN, simul sumptis multo major. Trianguli igitur AGN latus AN duobus reliquis simul sumptis majus. Quod est absurdum. Recta igitur AC recta CH major non est. Sed nec æqualis. Minor igitur. Simili ratione et aliâ omni minor, quæ arcum BD contingens contingentibus primo positis AB, CD intercepta est. Omnium igitur minima.

*Q. E. D.*

### THEOREMA III.

*Polygonorum omnium, lateribus numero datis, datum circum-  
lum circumscribentium, æquiangulum perimetro mini-  
mum est.*

CIRCULUM ABCD circumscriptum puta polygono, quot volueris laterum, FGHKE, quod omnium quæ, æquali laterum numero, circa eundem circumscriptum circumscribi possunt, perimetrum minimam habeat. Dico polygonum FGHKE æquiangulum esse. Nam si æquiangulum non sit, necesse est ut duos aliquos angulos proximos inæquales habeat: nam si nullos proximos, omnino nullos; sed æquiangulum erit. Sunt igitur inæquales proximi duo anguli GFE, KEF. Latera vero GF, KE, quæ cum intermedio FE angulos illos complexa sunt, circum in A, D punctis contingant: et latus intermedium FE eundem in L contingat. Circuli centrum esto O. Jungantur OA, OL, OD. Anguli AFL, AOL simul sumpti duobus rectis æquales sunt, propter angulos ad A, L rectos. Similiter anguli

anguli  $DEL$ ,  $DOL$  simul sumpti duobus rectis æquales; propter angulos ad  $D$  et  $L$  rectos. Duo igitur  $AFL$ ,  $AOL$ , simul sumpti, duobus  $DEL$ ,  $DOL$  simul sumptis æquales. Inæqualibus igitur  $AFL$ ,  $DEL$  hinc inde ablati, relinquuntur  $AOL$ ,  $DOL$  inæquales. Arcus igitur  $AL$ ,  $LD$  inæquales. Bisariam igitur secetur arcus  $ALD$  in  $M$  puncto, quod necessario à puncto  $L$  diversum erit. Per  $M$  ducatur recta quæ arcum  $AD$  contingat. Contingens per  $M$  contingentibus  $AF$ ,  $DE$  in punctis  $N$ ,  $P$ , occurrat. Recta  $NP$  recta  $FE$  minor erit (per præc). Quare et dupla rectæ  $NP$  duplâ rectæ  $FE$  minor. Sed propter  $NM=NA$  et  $PM=PD$ , tres rectæ  $AN$ ,  $DP$ ,  $PN$ , simul sumptæ, duplæ rectæ  $NP$  æquales sunt. Et propter  $FA=FL$ , et  $DE=EL$ , tres  $AF$ ,  $DE$ ,  $FE$ , simul sumptæ, duplæ rectæ  $FE$  æquales sunt. Tres igitur  $AN$ ,  $DP$ ,  $PN$  simul sumptæ tribus  $AF$ ,  $DE$ ,  $FE$ , simul sumptis minores. Polygonum autem  $NGHKP$ , circulum  $ABCD$  circumscribit, et latera numero totidem habet ac polygonum  $FGHKE$ ; quod omnium quæ æquali laterum numero, circulum  $ABCD$  circumscribunt, perimetro, ex hypothese, minimum est. Perimeter igitur  $FGHKEF$  perimetro  $NGHXPN$  minor. Utrique auferatur pars communis  $AGHKD$ : relinquentur  $AF$ ,  $DE$ ,  $FE$ , reliquis  $AN$ ,  $DP$ ,  $PN$ , simul sumptæ simul sumptis, minores. Ast majores jam offensæ sunt. Simul igitur majores et minores. Quod est absurdum. Non sunt igitur anguli  $GFE$ ,  $KEF$ , inæquales, existente perimetro  $FGHKEF$  minimâ. Similiter ostendetur, nec alios duos quosvis angulos proximos polygonum  $FGHKE$  inæquales habere.



Nullos igitur proximos inæquales habet. Omnino igitur nullos. Omnes igitur æquales. Æquiangulum igitur. Q. E. D.

#### THEOREMA IV.

*Polygonorum omnium, lateribus numero datis, datum circum circumfcribentium, æquiangulum areâ minimum est.*

PATET ex præcedente, cum area æqualis est re-angulo sub femiperimetro et circuli inſcripti femidia- metro.

#### THEOREMA V.

*Polygonorum omnium, lateribus numero datis, dato circulo inſcriptorum, æquilaterum perimetro maximum est.*

CIRCULO ABCD inſcriptum puta polygonum ABCDE, quot volueris laterum, quod omnium, quæ, æquali laterum numero, eidem circulo inſcribi poſſunt, perimetrum maximam habeat. Dico polygonum ABCDE æquilaterum. Non enim. Duo igitur proxima quædam latera inæqualia habet: nam ſi nulla proxima, omnino nulla; ſed æquilaterum erit; quod negaſti. Inæqualia ſunt latera proxima AB, AE. Propter rectas AB, AE inæquales, arcus AB, AE inæquales erunt. Bifariam igitur ſecetur arcus

BAE in puncto G, quod à puncto A diversum erit. Jungantur BG, EG. Circuli centrum esto F: juncta GF peripheriæ iterum in H occurrat. Denique jungantur BE, HA. Arcubus GB, GE æqualibus, semicirculis GBH, GEH, ablati, relinquuntur æquales BH, EH. Quare et anguli BGH, EGH, nec non BAH, EAH, æquales. Angulos igitur BGE, BAE, qui in eodem sunt segmento circulari, rectæ GH, AH, bifariam dividunt. Duæ igitur GB, GE, simul sumptæ ad rectam GH eandem proportionem habent, ac duæ BA, AE, simul sumptæ, ad rectam AH. (*Vide Demonstrationem prop. 94. Datorum EUCLIDIS*) In circulo autem ABCD, diameter GH, recta AH, major est. Duæ igitur GB, GE, simul sumptæ, duabus AB, AE, simul sumptis, majores (per xiv. 5. Elem.). Polygonum autem GBCDE circulo ABCD inscriptum est, et latera numero totidem habet ac polygonum ABCDE; quod omnium quæ, æquali laterum numero, circulo ABCDE inscribi possunt, perimetro, ex hypothese, maximum est. Perimeter igitur ABCDEA perimetro GBCDEG major. Utrique auferatur pars communis BCDE. Relinquuntur AB, AE, reliquis GB, GE, simul sumptæ simul sumptis, majores. Duæ autem GB, GE, duabus AB, AE, ostensæ sunt majores. Simul igitur majores et minores. Quod est absurdum. Non sunt igitur latera AB, AE, inæqualia. Simili modo ostendetur, de aliis quibuscumque polygoni lateribus proximis, inæqualia non esse, existente perimetro maximâ. Nulla igitur proxima inæqualia. Omnino igitur nulla. Omnia igitur æqualia. Q. E. D.

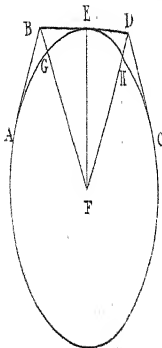
## T H E O R E M A VI.

*Polygonorum omnium, lateribus numero datis, dato circulo inscriptorum, æquilaterum area maximum est.*

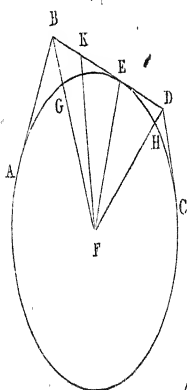
Demonstrationem vide apud THOMAM SIMPSON in libello suo de *Figuris Geometricis maximis et minimis*.



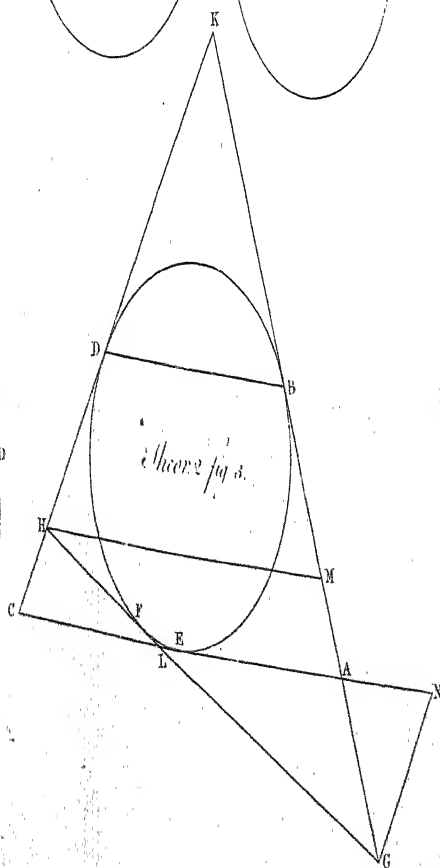
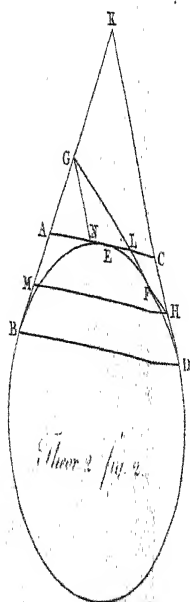
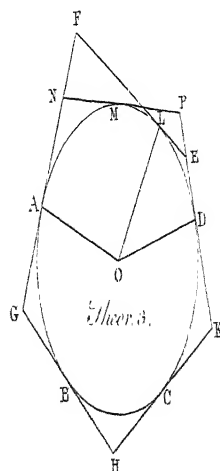
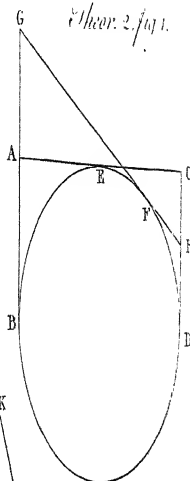
Theor 1 fig. 1.



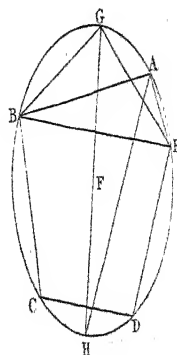
Theor 1 fig. 2.



Theor 2 fig. 1.



Theor 3







XXX. *An Account of an extraordinary acephalous Birth.*

*By W. Cooper, M. D. in a Letter to William Hunter, M. D. F. R. S.*

DEAR SIR,

Redde, April 27, 1775. **I** TAKE the liberty to remit you an account of the delivery of a very curious acephalous monster, accompanied with a short description of its anatomical structure. If, after a perusal of it, you should apprehend it may be acceptable to the Royal Society, I beg that you will do me the honour to lay it before them.

Mrs. BRACKETT, of Clerkenwell Close, aged twenty-three years, was, at the end of her first pregnancy, by a natural labour, delivered of a perfect female child, on Friday the 8th of October, 1773, at seven o'clock in the morning. The attending midwife, Mrs. AYRES, soon perceived by the abdominal tumour that there was another child. After waiting about three hours, a flooding came on; but without pain, or any advancement of the second delivery. The *hæmorrhage* producing faintness, debility, and danger, the attendants and midwife were alarmed, and I was sent for. When I came, I found her in the situation above described; and therefore



thought it my duty to accomplish the remaining part of the labour, as soon as I could, consistently with the safety of the mother. . . . Upon all occasions, when the concomitant circumstances render it necessary to turn a child *in utero*, it is of the utmost consequence, to understand, as nearly as we can, its general situation, in order to deliver with the greater ease, safety, and expedition. And to an experienced *accoucheur*, if the breech, knees, or feet, do not immediately present themselves, the head and face of the child will, in most cases, be a sufficient index to the position of the other parts of its body. This circumstance arises from the *fœtus* commonly coiling itself up into an oblong, oval, snug, compact figure, with its knees towards its chin, in order to take up as little room as possible, by being adapted to the cavity of the *uterus*. . . . In the present case, when the patient was placed in a proper situation, having introduced my hand as gently as possible through the *vagina, cervix uteri*, and enveloping membranes, and no part of the inferior extremities, or breech, presenting itself, I examined carefully for the head of the child, as usual, but without success. This disappointment somewhat embarrassed me. But as the woman's situation was become very serious by the increasing *uterine hæmorrhage*, I attempted without delay to get at the feet. I easily secured one of them; but though I made use of very little force in bringing it towards the *os externum*, the structure was so very tender that the *tibia* began to give way at its superior *epiphysis*. On this account I was reduced to the disagreeable necessity

sity of again introducing my hand into the *uterus*; and as one leg had thus unexpectedly failed me, I thought it extremely futile to attempt any thing with the other. The most eligible resource which I apprehended I had now left, was to fix a blunt hook upon one groin, and, when it was brought low enough, to assist gently at the other, with the two fore-fingers of my right hand. By these means I happily accomplished the delivery of the remaining *fœtus*, which proved to be a very singular kind of monster. And as the late ingenious Mr. HEWSON injected its blood vessels, and dissected it, I am enabled to attempt a short anatomical description of it, for the satisfaction of the curious in philosophy and physiology.

This extraordinary animal production is of the size and appearance of a common twin child at its full time, excepting the particularities now to be pointed out. When first born it was very plump, but soft and flabby, and the bones remarkably small and tender. It has neither head, neck, hands, or arms. In the place where the neck should originate, is a little *mamilla*, somewhat larger than a woman's nipple, but quite soft. And on each side, in the place where the arm should begin, there is a small *papilla*, about the bigness, and very much like the extremity, of a common quill. The spine seems perfect, but ends abruptly at the upper *vertebræ colli*. Below the navel the parts are nearly intire, except the feet, where the toes are of an irregular form and size, and some of them united together. The external parts of generation, which indicate it a female, are also perfect.

Upon a careful inspection internally, there is evidently no brain nor spinal marrow. A few nerves, however, are scattered about the *abdomen*; but their origin, for fear of destroying the preparation, is not traced. The *uterus* is perfect; but only one *ovarium* could be found. There is also the appearance of a bladder; but it is so contracted as to have no cavity. A large intestine arises from the *anus*; is a good deal convoluted when it gets above the brim of the *pelvis*, and ends in a blind pouch or *cul de sac*, on the left side of the *abdomen*. This *viscus* appears to be about six or seven inches in length, varies its size in different parts, gradually becomes smaller towards its superior extremity, and seems fully distended with a colourless *mucus*(<sup>a</sup>). All above the navel is extremely defective. There is no heart, lungs, diaphragm, stomach, liver, kidneys, spleen, *pancreas*, nor small intestines. However, there are three small glands in the place of the *thymus*, whose substance, when examined with a microscope, Mr. HEWSON remarked, exactly resembled that of the *thymus* itself. And on each side of the *vena cava*, just under the navel, are two little glandular substances, which seem to be somewhat like *capsule renales*, only very small to what are commonly

(a) Does not this circumstance almost amount to a proof, that the *meconium*, universally found in the bowels of new-born children, is nothing more than the *mucus* naturally secreted by the intestinal glands, mixed with bile, and perhaps a small portion of the *pancreatic* juice? In the present instance, as there is no liver there could be no bile, and consequently the *meconium*, if I may so call it, is colourless.

found<sup>(b)</sup>. There is a large artery running upon the spine, which may be called the *aorta*. As this approaches the upper extremity of the little animal, it is divided into smaller and smaller branches: and in its course it distributes lateral ones also to the contiguous parts of the trunk. Below the navel it sends off two branches that constitute the umbilical arteries, one of which is considerably larger than the other. And then below those, two other branches descend to the inferior extremities. A large umbilical vein comes in at the navel, and is immediately divided into two considerable branches; one ascending, the other descending. Each of these is again sub-divided into smaller and smaller branches, which, as they pass upwards and downwards, seem to correspond with the different ramifications of the ascending and descending *aorta*. The *funis umbilicalis* was only about two inches in length<sup>(c)</sup>, and so very tender also, that it unavoidably separated near the navel of the child during the delivery. Whether, therefore, there was any pulsation in this short *funis* I am not able to determine. The *placenta* was not particularly examined.

(b) Mr. HEWSON, some time before his death, seemed to be confirmed in the opinion, that whenever children are born with little or no brain, the *capsulae renales* are always very much diminished. This is certainly the case in one or two almost brainless children which I have by me, and whose *renal capsules* he examined, with a view of being further satisfied upon this subject.

(c) An exactly similar circumstance to this I took particular notice of, in the delivery of another almost brainless monster.

Here are evidently in this *fœtus* two distinct systems of vessels, arteries and veins<sup>(a)</sup>, that carried red blood<sup>(c)</sup>. It is plain also, that the blood passed from the internal *iliac* arteries, through the *hypogastrics* and *umbilicals* to the *placenta*, and was returned from it by the umbilical vein to the navel, and thence distributed in the manner before observed. But as there is no heart, nor any thing analagous to one, it becomes extremely difficult to ascertain the powers by which the circulation was carried on through this curious physiological phænomenon. May we not however venture to advance a conjecture, that the peristaltick, or living muscular power of the arteries, was principally subservient to this important end? Many examples are to be met with in the collections of the curious and learned in the different parts of Europe<sup>(d)</sup>, which

(d) Mr. HEWSON attempted to inject the whole blood vessels by the umbilical vein, as usual. To his great surprize, no part of the injection returned by the umbilical arteries. He could not account for this singularity at that time: but as only a part of the vessels were filled, he injected afresh by one of the *hypogastric* arteries. Upon dissection afterwards, this mystery was unravelled by the heart's being totally absent. It then appeared also, that by the first injection he had filled the venal system, and by the latter the arterial.

(e) See a very curious case related by Mons. WINSLOW, in the *Memoires de l'Académie des Sciences* for 1740, p. 586. and 604. Among other remarkable singularities in this little monstrous abortion of six months, that excellent anatomist particularly takes notice, that there was no appearance of one drop of red blood in any of its vessels, which were universally filled with a serous lymph; and that there were no vestiges of any veins at all.

(f) F. LICETUS, de *Monstris*, p. 300. et seq. PALFYN, *Traité des Monstres*, p. 325. CHESLDEN's *Anatomy*, 5th ed. p. 379. *Philosophical Transactions*, 1739-40, N° 456. p. 303. Ibid. 1767, p. 1. *L'Académie des Sciences, Hist.*

which are somewhat similar to that now related. When carefully examined, however, excepting a very few instances, they are generally found either essentially to differ, or else their structure has not been, with any tolerable precision, explained. The present history affords also an exception to a frequent remark among authors, "That brainless children are always very brisk before they are born;" for the mother has frequently told me, "That she felt no motion at all within her after the first birth; and that she had not the least suspicion of there being a second child till it was delivered." This circumstance may however, perhaps, be attributed to the *medulla spinalis* being totally deficient, as well as the *cerebrum* and *cerebellum*.

Physiologists and philosophers have spent a great deal of time in attempting to investigate the causes of these extraordinary phænomena. With this view many opinions have been started; but most, if not all of them, as far as I am able to judge, being built upon the tottering basis of conjecture only, afford, upon an attentive inspection, but little satisfaction to a dispassionate enquirer after truth. The particular hypothesis, which has been almost universally adopted, is, that monstrosity and marks in children depend upon the imagination and

1720, p. 13. Ibid. Mem. 1720, p. 8. Ibid. 1740, p. 586. and p. 592. Miscellanea Curiosa Ephemeridum Germanicarum Ann. XIX, p. 258. Acta Eruditorum Lipsæ, Ann. 1724, p. 501.

(g) Philosophical Transactions, 1674, N° XCIX. p. 6157. Ibid. 1767, p. 18.

longing

longing of the mother. Such pernicious a principle as this ought to have very rational evidence, and the most striking facts to support it. But is it not directly the contrary? Indeed a great many ridiculous stories have been related to the world<sup>(2)</sup>, which, however, upon a little reflection either obviate themselves, or else are contradicted by those facts that occur. May we not exemplify this observation by the case of twins now related? One of the children was perfect, and is still living; the other proves to be remarkably defective. Does not the question naturally arise here, how could one child be affected by the disturbed imagination of the mother and not the other? But the mother, upon repeated examination, recollects no fright in particular while she was pregnant. Neither, if she did, would it all invalidate the force of our argument upon this subject; for she could not possibly see any child without a head: and more especially, because other parts, as the *viscera* and *medulla spinalis*, were equally defective, which are entirely out of the reach of the eye or imagination of the mother to form any idea about them. To elucidate this point still further, can any candid person possibly suppose, that the casual agitation of mind of a pregnant woman, should either produce or destroy a whole system of blood-vessels, nerves, and fibres, which are indispensable constituents of almost every part of the

(2) MAURICEAU, p. 53. Obs. 64. Ibid. p. 63. Obs. 118. SMELLIE'S Midwifry, vol. III. p. 402. Philosophical Transactions, 1684, N<sup>o</sup> 160. p. 599. Ibid. 1739-40, N<sup>o</sup> 456. p. 303. and 306.

body? And may we not adduce one proof more, in support of our argument, from what happens to animals and vegetables? Among these also, such extraordinary deviations from the general course of nature are by no means uncommon: yet the former are possessed of a much less share of imagination than is generally allotted to the human species; and the latter have none at all. Reasoning in the same manner upon several occasions of this kind in which I have been concerned, my conclusions have always been similar; viz. that the usually assigned cause of the mother's imagination is by no means equal to the manifold effects produced. And on the other hand, this injurious doctrine is pregnant with continual mischief to society. It frequently makes women very unhappy. And the fear of mutilating or marking their infants often affects them so much, that they at last miscarry. Having therefore indubitable facts to go upon, and the cause of humanity so powerfully coinciding with the truth, is it not right to affirm and maintain with confidence, that neither the longing nor frightened imagination of the mother appears to have any power at all to imprint marks or monstrosity upon children? That this is a very weak supposition, entirely void of foundation, directly contrary to all philosophy and experience, and has nothing to support it but a vulgar opinion, transmitted to us from the ages of anatomical ignorance? And is it not more reasonable to conclude with you, SIR, in your extensively useful lectures, that whatever be the defect or deformity in a monstrous birth, it can never be



occasioned by accidents of any kind during pregnancy; but probably has its existence always originating, *causâ obliâ incognitâ*, in the first stamina of the embryo<sup>(1)</sup>.

Thus, SIR, I have faithfully related the particulars of this singular phænomenon among the human species, which, to a demonstration, confirms your opinion, that the nourishment of the *fœtus in utero* is principally by means of the *funis umbilicalis*. M. MERRY observes, that defective monsters are more instructive than others that have redundancies<sup>(2)</sup>. If this be true, here is still an ample field for speculation, notwithstanding the few very obvious remarks which I have already ventured to make. In conformity to the general language of authors, I have in this essay occasionally adopted the use of the term *monster*. There is, however, something in that word extremely repugnant to our common feelings, and very apt to leave a terrifying impression upon the mind. Why may not the Author of Being sometimes produce variations in the human species, as well as in the animal and vegetable kingdoms<sup>(3)</sup>, and equally exempt too from such frightful appellations? Would it not, therefore, be more eligible in the present instance, and every similar one, to explode the common term, and call it simply

(1) The great baron HALLER is of opinion also, that this is evidently the case in that species of monsters to which parts are added. Vide *Opera Minora HALLERI*, tom. III. p. 148.

(2) L'Académie des Sciences, Hist. 1720, p. 13.

(3) See F. LICETUS. J. PALFYN des Monstres, &c. in which are many instances of each kind.

a *luxus nature*; or with PLINY to say, “ *Hoc nobis miraculum, sibi ludibrium, ingeniosa finxit natura.*”

The peculiar share of your friendship, with which you are pleased to honour me, makes me flatter myself that you will give me credit for the truth of any facts advanced in the course of the detail, that are not irrefragably evidenced by a recourse to the preparation in its present state.

I am, &c.

Norfolk-street,  
June 6, 1774.

XXXI. *Observations on the State of Population in Manchester, and other adjacent Places, concluded. By Thomas Percival, M. D. F. R. S. and S. A. Communicated by the Rev. Dr. Price, F. R. S.*

Redde, Feb. 2,  
1775.

A VERY accurate survey was completed last year, of the towns of Manchester and Salford, with their respective townships. This spring an enumeration, equally exact and comprehensive, has been made of the whole parish of Manchester, which contains thirty-one townships (exclusive of the two above mentioned) in the compass of less than sixty square miles. The reader is here presented with the particulars of this enumeration.

Tenanted houses,	2371	Under 15,	5545
Families,	2525	Above 50,	1762
Inhabitants,	13786	Above 60,	470
Males,	6942	Above 70,	261
Females,	6844	Above 80,	87
Married,	4319	Male lodgers,	68
Widowers,	23	Female ditto,	51
Widows,	315	Empty houses,	41

The number of persons to a house in Manchester is therefore nearly  $5\frac{4}{5}$ ; of individuals to a family, about  $5\frac{1}{2}$ ; and

and  $\frac{1}{5}$ th of the inhabitants have attained the age of 50. It is unnecessary to point out the difference in the proportions between the town and the adjacent country, as it will appear sufficiently obvious by comparing this account with that of Manchester. The whole number of the inhabitants in the town, township, and parish of Manchester amounts to 42937.

At the close of 1772, an account was collected from every country chapel, both episcopal and dissenting, in the parish, of the baptisms and burials of that year. The former were found to amount to 401, the latter to 246; and there is a presumption that this is nearly the annual proportion of deaths in the parish of Manchester, exclusive of the town and township. For the number of burials in the whole parish was, in the same year exactly 1200; and it has been shewn, that the deaths in the town of Manchester are, one year with another, 958. This sum being subtracted from 1200, leaves a remainder (242) for the country, very nearly equal to 246. And if 13786, the number of people in the parish, be divided by 246, it will appear, that only 1 in 56 of the inhabitants dies annually, whilst the yearly mortality in Manchester is as 1 to 28. Such a striking disparity in the healthiness of a large town, and of the country which surrounds it, granting it to be less than has been supposed, will scarcely be credited by those who have paid no attention to enquiries of this nature. And it must afford matter of astonishment even to the physician and philosopher, when he reflects that the inhabitants of both live in the same climate,

mate, carry on the same manufactures, and are chiefly supplied with provisions from the same market. But his surprize will give place to concern and regret, when he observes the havoc produced in every large town by luxury, irregularity, and intemperance<sup>(a)</sup>; the numbers that fall annual victims to the contagious distempers, which never cease to prevail; and the pernicious influence of confinement, uncleanness, and foul air on the duration of life<sup>(b)</sup>.

“ It is not air, but floats a nauseous mass

“ Of all obscene, corrupt, offensive things.”

ARMSTRONG on Health, book I.

Great towns are in a peculiar degree fatal to children. Half of all that are born in London die under three, and in Manchester under five years of age; whereas at Royton, a manufacturing township in the neighbourhood of Manchester, the number of children dying under the age of three years, is to the number of children born only as one to seven; and at Eastham, a parish in

(a) There are at this time in Manchester no less than 193 licensed houses for retailing spirituous and other liquors, and 64 in the other townships of the parish. At Birmingham, the number of public houses is still greater than at Manchester. A very ingenious friend of mine in that place has computed, that the quantity of malt consumed there in the public houses, requires for its growth, a compass of land which would be sufficient for the support of 20,000 men.

(b) The rev. Dr. Tucker, dean of Gloucester, informs me, “ That were it not for the daily arrival of recruits from the country, his parish (St. Stephen’s in Bristol), and indeed Bristol in general, would be left in a century without an inhabitant, unless the people would betake themselves to better courses.”

Cheshire,

Cheshire, inhabited by farmers, the proportion is considerably lessened.

It is a common but injurious practice in manufacturing countries, to confine children, before they have attained a sufficient degree of strength, to sedentary employments, in places where they breathe a putrid air, and are debarred the free use of their limbs. The effect of this confinement, says an able writer, is either to cut them off early in life, or to render their constitutions feeble and sickly; but the love of money stifles the feelings of humanity, and even makes men blind to the very interest they so anxiously pursue. The same principle of sound policy which induces them to spare their horses and cattle, till they arrive at a due size and vigour, should determine them to grant a proportionable respite to their children<sup>(c)</sup>. And this observation may, perhaps, be extended to the untimely culture of the mind. For too early an application to study impairs the faculties, injures the constitution, and hurts the temper by frequent contradiction. Almost as soon as a boy has acquired the powers of speech, he is shut up many hours every day in a noisome school, secluded from the benefit of exercise and the refreshment of the open air, and tied down to the severe drudgery of learning what serves only, at such a period of life, to over-charge his memory,

(c) See my former paper on Population. Phil. Trans. vol. LXIV. part 1. page 62.

(d) See Dr. GREGORY's Comparative View of the State and Faculties of Man.

and to destroy his native cheerfulness of disposition. Thus the age of gaiety (to use the words of the elegant writer before referred to) is spent in the midst of tears, punishments, and slavery; and this to answer no other end, but to make a child a man, some years before nature intended he should be one.

The reverend Mr. HARRISON, of Chapel in le Frith, has made a survey, at my request, of the inhabitants of Chinley, Brownside, and Bugfworth, three hamlets contiguous to each other, in the parish of Glossop and Peak of Derbyshire. They are four statute miles in length, and three in breadth; and contain 301 males, 310 females; 200 married persons, 15 widowers, 18 widows; 234 persons under fifteen years of age, 121 above 50, and 9 who have attained the age of 80. This enumeration was finished in September 1773.

I have been furnished by the reverend Mr. ASHETON, rector of Middleton, near Manchester, with an account of the births, deaths, and marriages, in his parish, during ten corresponding years of the last and the present century. From 1663 to 1672 inclusive, the deaths were males 180, females 187; the births, males 200, females 188; the marriages 121. The births, therefore, during ten years, only exceeded the deaths in number 21. And the average number of births to each marriage was as  $3\frac{1}{3}$  to 1. From 1763 to 1772 inclusive, the deaths were 499 males, 494 females; the christenings, 802 males, 768 females; the marriages 330. The baptisms, therefore, during this period, exceeded the deaths 577; that

that is, near 58 annually. And if no allowance be made for illegitimate births (which, I believe, in this parish are not numerous, and can no where be supposed to equal one fourth of all that are born), each marriage has produced  $4\frac{3}{4}$  children. It is curious to observe the change in the proportion of births to the deaths, and also to the marriages, which has taken place at Middleton (and I have received similar accounts of other places), during the course of the last century. The former may be explained by the greater encouragement to matrimony from the increase of trade; the latter is of more difficult solution; though it is probable, that the warmer cloathing and better fare, which the poor now enjoy, may have contributed to it. Luxury, when carried to such a degree as to enervate the constitution, is unfavourable to population; but plenty of nutritive diet may well be regarded as a source of fruitfulness. The lower class of people in this county formerly lived upon the coarsest food. Wheat, an hundred years ago, was almost unknown to them; and so lately has it been cultivated in Lancashire, that it has scarcely yet acquired the name of corn, which in general is applied only to barley, oats, and rye. Potatoes also are much improved by the present judicious method of growing and propagating them; and they now constitute a most wholesome and nourishing part of our diet. Perhaps likewise the general use of pepper and of other spices may increase the fertility of mankind. But I shall suspend my conjectures for the present. A variety of causes may counteract the

VOL. LXV.                      V y                      operation



operation of those which I have enumerated, and a considerable number of facts must be adduced, to ascertain, whether the proportion of births to marriages be generally increased in countries advanced from poverty to wealth, by the introduction of trade, or the improvements of agriculture. The instance of Middleton and of one or two places more which first occurred to me, and suggested the preceding observations, is opposed by others which have lately fallen under my notice. And I cannot close this subject better than by giving a view of the facts which I have collected on both sides the question.

A TABLE shewing the proportion of births to marriages in different places, and and at different periods of time.

Middleton.

Year.	Marriages.	Christenings.	Births to a Marriage.
From 1663 to 1672	121	388	$3\frac{1}{5} \div$
1763 1772	330	1570	$4\frac{3}{4}$

Warrington.

From 1702 to 1722	131	385	$2.9 \div$
1752 1772	1549	5034	$3\frac{1}{4}$

Pentraeth Parish, Anglesey (c).

From 1740 to 1747	32	100	$3\frac{1}{8}$
1764 1771	33	149	$4\frac{1}{2}$

(c) Vide Philosophical Transactions, vol. LXIII.

## Llanddyfnan Parish, Anglesey (C).

Year.	Marrages.	Christenings.	Births to a Marriage.
From 1750 to 1757	28	111	3.9+
1764 1771	32	154	4 $\frac{2}{3}$ +
1547 1554	8	36	4 $\frac{1}{2}$
1620 1627	20	44	2 $\frac{1}{3}$

## Liverpool.

From 1700 to 1710	500	2127	4 $\frac{1}{2}$
1762 1771	4812	10010	2 $\frac{1}{12}$

## Bowden.

From 1653 to 1662	136	573	4 $\frac{1}{3}$ +
1763 1772	369	1300	3 $\frac{1}{2}$ +

I have lately received, from the reverend Mr. Archdeacon Blackburn, rector of Richmond, in Yorkshire, the following account of his parish. From the year 1764 to 1773 inclusive, 452 males and 376 females have been baptized; 299 males and 341 females have been buried. The marriages, during this period, have amounted to 200. In Richmond there are about 600 houses; but the Easter book enumerates only 450 families, and Mr. BLACKBURN computes the number of inhabitants to be 2300. “ We have no distempers, he says, “ that can be called endemial; and when fevers prevail “ in the neighbourhood, few are affected by them in “ this town. If any person brings an ague to Rich- “ mond, he is generally freed from it in a few days;

(C) Vide Philosophical Transactions, vol. LXIII.

“ though the village of Gilling, about a mile and a half  
 “ distant, which stands low, and has a large pool of stag-  
 “ nant water adjoining to it, is visited with this com-  
 “ plaint every spring and autumn. The air of Rich-  
 “ mond seems to be peculiarly unfavourable to con-  
 “ sumptive disorders. Many strangers come hither from  
 “ different parts in the first stage of the *phthisis pulmo-*  
 “ *nalis*; but after thirty-five years experience, I may  
 “ truly say, that not one has recovered, although the ut-  
 “ most care and attention have been paid to their respective  
 “ cases. The natives and constant residents are not sub-  
 “ ject to distempers of the lungs, except when brought  
 “ on by intemperance. But rheumatic complaints are  
 “ very general, especially amongst the senior part of the  
 “ inhabitants. In small corporation towns, like Rich-  
 “ mond, numbers are taken off by excessive drinking;  
 “ but the people here, who live temperately, seldom die  
 “ earlier than in their eightieth year.”

Happening to pass through Sutton Coldfield, in War-  
 wickshire, last summer, I was very much struck with the  
 beauty and apparent healthiness of its situation, and was  
 desirous of knowing the duration of life which the in-  
 habitants of it enjoy. The rector of the parish has, with  
 great politeness and good-nature, gratified my curiosity  
 as far as he is able, by furnishing me with an extract  
 from the church register, and by referring me to the  
 XXXIId volume of the Gentleman's Magazine for the  
 following authentic account of the place, drawn up, I  
 suppose, by himself.

“ Sutton Coldfield is almost full South of Litchfield,  
 “ at the distance of about eight measured miles, by  
 “ which it undoubtedly got its name of Sutton, a con-  
 “ traction of South Town. A remarkable bleak and  
 “ barren common, which lies directly West of it, just  
 “ out of the bounds of the parish, might probably give  
 “ it the additional denomination of Coldfield. The air  
 “ upon that heath, as travellers have declared, is as keen  
 “ and cold as that upon the Highlands of Scotland. The  
 “ parish is nearly oval in its figure, the longest diameter  
 “ seven miles, and the breadth four. The face of it is  
 “ agreeably diversified with gently rising hills, and val-  
 “ lies of tolerably fruitful meadows. It is bounded on  
 “ the North by Shenston, on the West by Barr, on the  
 “ South by Curdworth and Aston near Birmingham,  
 “ and on the East by Middleton. It contains four ham-  
 “ lets; *videlicet*, Mancy, Hill, Little Sutton, and Warm-  
 “ ley. In the year 1630, there were 298 houses in the  
 “ parish; in 1698 there were 310; in 1721 the num-  
 “ ber was increased to 360; which is nearly about the  
 “ number at present. I compute the inhabitants at  
 “ 1800. The register begins in the year 1603. The  
 “ number of christenings for the first twenty years of  
 “ the register was 645; the burials during the same pe-  
 “ riod were 501. The number of christenings for the  
 “ last twenty years (ending at Christmas 1761) was  
 “ 747; the burials 694(s).” If the number of inhabi-

tants in this town be rightly computed, the yearly mortality amongst them is not so much as 1 to 51; and every house contains at a medium five persons.

It appears by the observations lately communicated to me by the rev. Dr. TUCKER, that the number of females baptized at the parish church of St. Stephen's, in Bristol, from 1754 to 1774, has exceeded the number of males baptized during the same period of time; and that the like remark has been made in some other parishes of that city. From these facts the learned dean concludes, that Mr. DERHAM's calculation, which supposes the proportion of male to female births to be as 13 to 12, may possibly be erroneous; and he expresses his earnest wishes, that farther enquiry may be made into a subject of so much importance. The following table will shew the result of the few observations which I have collected.

A comparative view of the number of males and females  
baptized in different places.

Places.	Males.	Females.
Dishley, 11 years,	149	145
St. Stephen's parish, Bristol, 20 years,	591	607
Taxal, 15 years,	204	230
Richmond, 10 years,	452	376
Middleton, 10 years,	200	188
Bowden, 10 years,	663	639
Middlewich, 5 years,	229	242
Chapel in le Frith, 10 years,	451	332
Warrington, 1 year,	175	181
Collegiate Church, Manchester, 7 years,	3215	3024
Royton, 10 years,	134	120
Chester, 2 years,	408	415
Total,	<u>6871</u>	<u>6499</u>

This table sufficiently confirms the calculation of Mr. DERHAM, with respect to the greater proportion of male than of female births; but the succeeding table evinces, that the number of females living considerably exceeds the number of males in a variety of places, and that the widows are almost double the number of widowers.

A comparative view of the number of males and females  
in different places.

Places,	Males.	Females.
Manchester,	10548	11933
Salford,	2248	2517
Townships of ditto,	947	958
Parish of Manchester,	6942	6844
Bolton,	2159	2392
Little Bolton,	361	410
Monton,	196	190
Hale,	140	136
Horwich,	149	156
Darwen,	900	950
Cockey,	320	391
Chawbent,	554	606
Ackworth,	340	388
Eastham,	451	461
Chinley,	181	168
Brownfide,	40	47
Bugfworth,	80	95
Total,	<u>26556</u>	<u>28642</u>

A comparative view of the number of widowers and widows in different places.

Places.	Widowers.	Widows.
Manchester,	432	1064
Salford,	89	149
Townships of ditto,	21	42
Parish of ditto,	232	315
Monton,	14	13
Hale,	8	12
Horwich,	9	8
Darwen,	30	48
Cockey,	10	27
Chowbent,	26	43
Chinley,	27	31
Brownside and Bugfworth,	15	18
Total,	<u>913</u>	<u>1770</u>

The reader will perceive, from these tables, that the proportion of males to females baptized, is nearly as 12 to 11 $\frac{1}{3}$ , or 19 to 18; but that the number of females living is to the number of males as about 11 to 10 $\frac{1}{5}$ , or more exactly as 14 to 13; and that the widows are almost double the number of widowers.



XXXII. *An Account of the Effects of Lightning on a House, which was furnished with a pointed Conductor, at Tenterden, in Kent. In Two Letters from Richard Haffenden, Esquire, the Proprietor of the House, to Mr. Henley. To which are added some Remarks by Mr. Henley.*

Redle, May 4,  
1775.

HAVING been informed by my friend Mr. CURTEIS, that the house of RICHARD HAFFENDEN, esquire, at Tenterden, in Kent, had been damaged by a stroke of lightning, notwithstanding it had been furnished with a metallic conductor to prevent those accidents: I wrote to Mr. HAFFENDEN, requesting the favour of the particulars, and especially an answer to the following questions.

1. The length and breadth of the house?
2. The height of the chimney, to which the conductor was affixed from the ground, and how far it might rise above the ridge or roof of the house?
3. To what height the conductor was carried up above the top of the chimney?
4. Whether it was terminated by a sharp point, a blunt end, or a ball?
5. Whether it was in regular contact from the top to the bottom?
6. What was its diameter or breadth?
7. Was it carried down into the moist earth, or water?
8. What is the nature of the soil it enered?
9. Did any other chimney, or stack of

chimnies, sustain a stroke, besides that which carried the conductor: and what was the distance of each of the stricken chimnies from the conductor? and was there any communication of lead on the ridge or roof of the house, that might connect such chimney, or part of the house in which a stroke was received, with the conductor?

10. What was the damage the house sustained; and how was the electricity conveyed to the earth? 11. Doth your house stand single?

12. Is it situated on a plain, or on an eminence?

13. Had it rained before the stroke; and was that part of the house on which the lightning fell, considerably wetted at the time?

14. Did the wind blow toward that part of the house?

15. When did the accident happen?

---

## L E T T E R I.

SIR,

**I** RECEIVED yours of the 31st ult.; in answer to which, please to observe the following sketch of the top of the house (*vide* TAB. VII. fig. B.). *a, b, c, d*, are four chimnies, about thirty-eight feet high, and two feet above the ridge of the house. *f* is the conductor, elevated about five feet above the top of the chimney: it is made of iron, about half an inch diameter, tapering to a point, gilt. After being fixed to the chimney, eight or ten feet, it turns under the roof to the leaden pipe (*g*), which carries the water from the gutters *E, E, E*, and goes down on the outside of the house, till it reaches within four feet of the earth.

earth. In the lower end of this pipe is put one end of a square rod of iron, about three quarters of an inch in breadth; the other end of the iron-rod rests on the ground, at the distance of six feet from the foundation. The soil is light mould. The house stands single, on the West side of a hill; the front toward the hill. On the top of the hill, at about forty rods distance, is a wind-mill, whose bottom is nearly as high as the top of the house. About two of the clock in the morning of June 17, 1774, after thundering for six or seven hours at a distance, and coming slowly on, there being little or no wind, there came three amazing strokes. It rained extremely at the time; but ceased immediately at the last stroke, which struck the top of the chimney (*b*) the furthest from the conductor, and broke it down to the lead (*b*), where it divided; some up and down the rafters (*i, i,*) breaking and splitting both tiles and rafters into thousands of pieces, and throwing some of them to a great distance, till it reached the gutter (*E*), which was full of water. Another part, or division of the explosion, broke the mortar down to the lead on the cornice (*k, k, k,*); which lead goes round the front of the house. Through this lead the lightning passed quietly, till it reached the chimney (*c*), and broke the mortar on that chimney, till it reached the lead adjoining to it; through which lead it passed quietly, and then broke the tiles to the lead on the chimney (*d*); and from thence it passed to the gutter or leaden pipe; and there, uniting with the other division of the explosion, it passed down the leaden pipe.

pipe to the iron bar at the bottom; where, by an explosion, it burst a hole in the pipe, in passing from one metal to the other; and then went down the iron rod to the ground, flowing with water. It seems to me, according to the situation and direction of the cloud, being nearer to the gutters than the conductor, that *they acted as conductors*; and that the conductor itself was not in the least affected by the stroke; and that had there been conductors on the chimnies (*a* and *b*) they would have conveyed the electric fluid to the ground, without any damage to the house: or had there not been a continuation of metal from the gutters, &c. to the earth, it would have damaged the house all the way down. There was another division of the explosion, which, I imagine, came down the chimney, and struck to a bell-wire in the passage, and destroyed the wire to its end, which happened to be just within-side the wall, where the water-pipe was without; and there it made a small hole, in the joint of the bricks of an eighteen-inch wall, to the pipe; and then passed to the earth with the rest. I think this accident is no proof whatever of the preference of balls or points as terminations of conductors. Thus, SIR, I have endeavoured to answer every one of your queries; which, if satisfactory, will give pleasure to, &c.

N. B. The pricked line in the figure shews where the lightning passed quietly, having a sufficient substance of metal to pass through. The *curved line* shews where it broke every thing. The diagonal lines are all lead.

## L E T T E R II.

SIR,

I HAVE received your two last; and can assure you, it is with great pleasure I answer any question you shall be pleased to ask. In answer to your first and second queries; *videlicet*, What is the breadth of the house? and what is the distance between the extreme part of the chimney (*b*) and the conductor? The drawing I sent you is a scale of one-tenth of an inch to a foot, or nearly so. 3d. The iron rod in the pipe is only a large *rusty spit* put in, a few inches, *pro tempore*. The place where the leaden pipe is bursten, is even with the point of the spit; very little melted, but broke open with the explosion. The pipe is bent outward, about a foot at the lower end, to carry the water from the house; the hole in which, and the point of the spit, are near the middle of that projecting part of the pipe. No visible alteration appeared on the rusty spit by the lightning having passed through it. The lower end of it lays *on*, not *in* the earth. I am, &c.

(*a*) Then, the breadth of the house is about twenty-nine feet, and the distance between the farthest corner of the chimney (*b*) and the conductor, about forty-nine or fifty feet. W. HENLEY.

THESE

THESE letters containing a satisfactory answer to the questions which precede them, the following remarks naturally present themselves. 1st, A sharp pointed conductor did not, in this instance, invite or draw down upon itself a stroke of lightning. 2dly, Such a conductor, elevated five feet above the top of one of the chimnies, to an house of this dimension, may not perhaps be sufficient, by its silent attractive force, to protect the whole of such a building from a stroke; especially when a chimney, *a blunt body*, wetted with the rain, standing at fifty feet distance from the conductor, and being within five feet of its height, is in actual contact with so large, though irregular, a communication of metal, leading from the chimney directly to the conductor; though, in this instance, it should be remarked, that the conductor itself *was not in contact* throughout; and it is, for that reason, a very exceptionable case. 3dly, Two such conductors; one, for instance, on the chimney (*d*), where this was placed; and the other, on the chimney (*b*), which was stricken, with a communication of lead between them, would probably have protected the house: but a conductor on each chimney would certainly have secured the whole building effectually (*b*). 4thly, As the three branches or divisions of the lightning all concentrated upon an iron bar, *three quarters of an inch square*, and produced no sign of heat in it, an iron bar of that size seems to be fully sufficient for the purpose. There ap-

(*b*) This method, I am informed, Mr. HAFENDEN hath lately adopted.

pears, however, to have been two defects in Mr. HAF-  
FENDEN's conductor: 1. The leaden pipe and the iron  
bar at the bottom were not in contact. 2. The iron bar,  
or a thick plate of lead, should have been continued down  
into the moist earth or water; and had not the earth, as  
Mr. HAFFENDEN observes, flowed with water, at the time  
of the accident, the want of this precaution might, per-  
haps, have been attended with some damage to the foun-  
dation. I have been thus particular in my inquiries  
into, and remarks upon, this fact, as I think it of great  
importance to those who erect conductors, to be inform-  
ed of every defect in the construction, and of the dan-  
gerous consequences thence arising.

P. S. In Mr. HAFFENDEN's second letter, he observes,  
that the bell-wire, mentioned in his first letter, was brass;  
and that so much of it as went through the passage was  
painted: and the painted part, he says, was not de-  
stroyed; but the paint was loosened on the wire, without  
being broken off, like the loose rind of a tree; which re-  
sembles the effect of the artificial electricity, in an ex-  
periment of Mr. KINNERSLEY's, where a wire was, by a  
great explosion, both lessened in diameter, and extended  
in length. The other part of the wire, which was not  
painted, except a short piece at the end, somewhat larger  
and of iron, was entirely melted. Query, If the wire  
before spoken of had passed through a stone, particu-  
larly a wet one, inclosing it firmly, would not that stone  
have been shivered to pieces?

XXXIII. *Of the Torpidity of Swallows and Martins.* By James Cornish, Surgeon, at Totness, Devonshire, in sundry Letters to the Honourable Daines Barrington, F. R. S. and Dr. Maty, Sec. R. S.

## L E T T E R I.

TO DR. MATY.

SIR,

Totness, Feb. 3, 1775.

Redde, May 25,  
1775.

**A**S it has long been a *desideratum* among the naturalists to decide, with certainty, whether swallows and martins remain in a torpid state during the winter, or are birds of passage; I shall make no apology for troubling you with this letter, as it determines one part of the question, as I imagine, beyond doubt. In the beginning of November, being fishing on the banks of the river Dart, which runs at the bottom of a very steep hill, from the side of which project several large rocks, overgrown with ivy and thicket; I was at once surprized with the sight of a great number of martins. Now the season of the year being so advanced, I desisted from my amusement, that I might the more carefully observe the birds, which, I concluded,



had been brought out of their winter quarters by the fineness of the afternoon, it being remarkably pleasant and warm for the time of the year; the Sun at that time darting its rays directly against the rocks, just opposite to which I had fixed my station. They continued to flit to and fro for near half an hour, keeping very near together, and never flying in a direct line above thirty or forty yards, and never, when at the farthest, above a hundred yards distant from the rocks; closer to which they now, as the Sun lowered, began to gather very fast. Their numbers now lessened considerably; and in a very short time they all returned into the fissures of the rocks, from whence they had been induced to venture out by the warmth of the evening. I was particularly careful to observe if there was a swallow amongst them; but there was not one. Of this I am certain; for they were several times within the distance of twenty yards from the places where I stood. I was the more attentive to this, as I had been repeatedly assured, by many masters of vessels in the fish-trade, that they constantly saw every autumn, as they sailed up the Mediterranean, vast flights of swallows, bending their course towards the South. From which there is the strongest reason to believe, that these birds, during the winter months, do seek a warmer climate; though Mr. BUFFON has, for want of positive evidence, left that point undetermined. The above account, of which I am at all times ready to attest the truth, settling the question, relative to martins, beyond any doubt, is the best apology I can make for the

the

the liberty I have taken in troubling you with it. If I have not been sufficiently circumstantial in this relation, I shall be at any time willing to answer any queries you may think proper to favour me with. I am, &c.

---

## L E T T E R II.

TO THE HON. DAINES BARRINGTON.

S I R,

March 31, 1775.

**I** SHOULD not have deferred a single post acknowledging the receipt of your favour of the 19th *ult.* if I could so soon have procured a second perusal of your very interesting paper, published in the LXIId volume of the Philosophical Transactions, in which you have thrown so much light on the subject, from the investigation of which I derive the honour of your correspondence. The season, as you observe, is arrived when swallows and martins do usually begin to make their appearance; but there has not yet been one seen in this part of the country, probably from the uncommon coldness of the season; but you may depend on my taking every method in my power to fulfil your request, so far as to get some of the birds shot, as soon as they begin to issue forth from the rocks, where they have undoubtedly remained in a torpid state since the month of November last. I am informed by a person who under-

stands blowing rocks, that it is almost impossible to come at the bottom of the fissures of these in question. It could not be done but by gun-powder, and then at a great expence, and not without danger of destroying much of the field above; besides, the force of the powder pervading every hollow, would inevitably annihilate the birds, and so frustrate the end of our labour. I conceived the idea of destroying the rock in its full force, at the time when the martins entered it, as I concluded, for their winter's dormitory; and believe, that had the weather continued favourable, I should have actually attempted it: but on reflection it did not appear, that the discovery would have been adequate to the difficulty and expence that would have attended it. For there is certainly nothing more extraordinary in finding martins, in a state of torpidity, than dormice or bats, which are animals equal in bulk to the swallow or martin. Dormice are frequently found dead to all appearance in the winter in old hedges; and we can procure bats at all times, in any number, from a subterraneous place, called Kent's Hole, near Torbay. Now if the examination of the intestinal tube of one of this tribe of mungrel animals in a torpid state, should be thought worth attending to, it can be done at any time. Bats, indeed, are sometimes seen in winter, in very mild weather; though none have yet made their appearance with us. And I am ready to attest, if occasion should require, that I have seen martins in Totness in the months of December and January; though I do not remember ever to have seen a  
swallow

swallow in the winter. Upon the whole, I cannot help thinking my own evidence, with respect to the martin, to be absolutely conclusive, as is likewise the testimony of Mr. STEVENS and Dr. PYE; though it is to be regretted, that these gentlemen should have left any doubt, whether the birds found in the mud were swallows or martins? And Mr. KLEIM, in his paper *De Hibernaculis Hirundinum*, asserts, that his father found three black martins or swifts in an old oak during the winter, which on being laid before a fire, soon recovered strength enough to fly about the room, though they died soon after. The objection which has been brought against the opinion, that these birds do remain torpid during winter, is, that all birds do moult once in a year, and swallows do not moult with us. Now this argument is of little weight with me; as I am of opinion, that no bird that is to remain in a torpid state during winter, can undergo the process of moulting; for it is probable, if I may hazard such a conjecture, that the great loss of blood, which other birds suffer during the change of their feathers, is saved by nature, in birds which undergo a state of torpidity, for their more effectual preservation in such a state. And I have known many instances of birds kept in cages that have not moulted for a season; particularly a fly-lark, which retained his song in full vigour during the autumn and all the winter. Attempts have been made to bring on a torpid state on the birds in question, by confining them in a cold cellar: but without success. The force of this objection seems to be

lost when we consider, that in this situation the birds must be in continual fear, and consequently not disposed to make that change, to which instinct has directed them, for their preservation and security; since all their attempts are to get out of confinement, as long as they have any spirit or strength left; and when these are exhausted, they die in course: and, I think, this state must be induced by a disposition in the animal itself, and cannot be brought on by compulsion. I hope I shall be successful in my endeavours to procure some of the martins at their first appearance; as the being any way instrumental to oblige you will be a very great satisfaction to, &c.

---

### L E T T E R    III.

TO THE HON. DAINES BARRINGTON.

SIR,

Totness, May 9, 1775.

**I** CAN make no other apology for having so long delayed writing to you, but a desire to get as much information, as would make my letter worthy your perusal; and if facts, as well established as the nature of things can well admit, are allowed to be of any consequence, I hope I have not entirely failed of success. Mr. TRIST, the present recorder, and late member of parliament for this town

town, assures me, that he once saw many martins in the winter, about Christmas, flying to and fro under a large rock, not more than a mile from Totneſs, and alſo near the river. Mr. DEVER, a reputable farmer, is ready at any time to make oath, that he once found a ſwift in the church of Ailſprington, in the middle of winter; that he took it in his hand, and though it ſhewed no ſigns of life, he is certain, it could not have been dead but a few hours. He ſuppoſes it dropped from the roof, at a time when ſome maſons were at work, repairing a breach. THOMAS DIDHAM alſo affirms, that he once ſaw, on the 26th of December, two ſwallows or martins, flying in a gentleman's court of Syfferton; that it was a pleaſant day; and that he then ſuppoſed that they had iſſued from the old thatch-covering of the out-houſes. But here follows a direct evidence, as to the torpidity of one kind of bird. Mr. WIAT made oath, laſt Sunday, in the pariſh church of Haberton, before me and a credible witneſs, that in the winter, and near Chriſtmaſs, he once found, in a hollow aſh-tree then taking down, a bird covered with a kind of down; that on handling it, it ſhewed ſigns of life; that the two labourers who aſſiſted in felling the tree, alſo handled it; that when they firſt perceived the bird, it appeared to be dead, yet the heat of their hands made it move briskly; and that this bird he believes to have been a cuckow. As the ſtory of the cuckow plucking off his feathers, and remaining torpid during the winter in hollow trees, is generally believed in this country, the eſtabliſhment of the fact appeared

peared to me of considerable importance; and if I have succeeded in this point, I hope you will have no objection to the manner of ascertaining it. I would wish to avoid a weak credulity on one hand, and obstinate scepticism on the other. Again, Mr. ACHARD, of Privy Garden, may be now living, to testify the truth of the account of the torpid martins, which he saw taken out of the banks of the Rhine, and which, in his letter to P. COLLINSON, esquire, read before the Royal Society, he so particularly describes. Now to prove the torpidity of birds, we have the presumptive evidence of Mr. TRIST, DEVER, DIDHAM, and myself; and the positive evidence of Mr. ACHARD, Dr. PYE, Mr. STEVENS, and Mr. WIAT; all men of character, and incapable of asserting an untruth. The opinion of ARISTOTLE, that some of the same species of birds do emigrate; and that others do pluck off their feathers, and remain torpid during the winter, cannot obtain credit: for we cannot suppose that these animals are governed by different instincts, in what immediately concerns their existence; but by the same universal law of nature, independent of their wills or inclination. But then the law of nature must be permitted to have its free course. Restraint destroys the rule of actions; and therefore, though M. BUFFON and others succeeded not in their attempts to bring on torpidity in swallows confined in cold cellars, yet I have pleased myself with the idea of confining young swallows, martins, &c. with the old ones; which may be easily taken at their nests when feeding, in a large walled garden, covered with a net or lattice,

in

in which there should be a pond. Probably the young birds, if the garden were large enough to find them food, would not be uneasy under their confinement. If this experiment succeeded, it would be curious to observe them in the different degrees of torpidity during the winter. The intestines might at different times be inspected, and their analogy with those of the torpid bat carefully observed. I have had an opportunity of examining the *viscera* of several torpid bats. The intestinal tube was perfectly empty, except about half an inch from the *anus*, where there was a little hard *feces*. The gall-bladder was filled with a pellucid, yellowish fluid. The ball of FAHRENHEIT's thermometer being laid in the body of one of them, the heat of the blood at the heart raised the quicksilver two degrees. In three others, opened at the same time, no heat could be perceived, either by the thermometer or by the touch. These experiments being made in the beginning of April, it is reasonable to suppose, that the bat which affected the thermometer, had begun to feel the approaching season. I think there is reason to believe, from the small quantity of *feces* in the intestines, and from its being so near the *anus*, that those animals, when they find themselves growing torpid, take sufficient food to serve them during the winter. All the animal functions in this state are carried on exceedingly slow; but that they do go on, in some degree, is evident from their emptiness, emaciation, and the *feces*, which are found in plenty underneath the place where they hang in clusters.



The birds, of the swallow tribe<sup>(a)</sup>, which I have procured, exhibit perfect plumage and extreme leanness; the intestines empty, except the gizzard, which contained a substance most like small twigs or straws. Swifts have not yet made their appearance with us; the first that can be procured shall be examined. The result shall be the subject of a future letter, when I hope once more to have the honour to subscribe myself, &c.

(a) N. B. I had desired him to shoot some of those which first appeared, and examine their intestines; as I also did with regard to the torpid bats.

D. BARRINGTON.

XXXIV. *Description and Use of a portable Wind Gage.*  
*By Dr. James Lind, Physician, at Edinburgh.*

Redde, May 11, 1775. **T**HIS simple instrument consists of two glass tubes AB, CD, of five or six inches in length (TAB. X. fig. 1.). Their bores, which are so much the better always for being equal, are each about  $\frac{4}{10}$ ths of an inch in diameter. They are connected together, like a siphon, by a small bent glass tube *ab*, the bore of which is  $\frac{1}{10}$ th of an inch in diameter. On the upper end of the leg AB there is a tube of latten brass, which is kneed or bent perpendicularly outwards, and has its mouth open towards F. On the other leg CD is a cover, with a round hole *c* in the upper part of it,  $\frac{2}{10}$ ths of an inch in diameter. This cover and the kneed tube are connected together by a slip of brass *cd*, which not only gives strength to the whole instrument, but also serves to hold the scale HI. The kneed tube and cover are fixed on with hard cement or sealing wax. To the same tube is foldered a piece of brass *e*, with a round hole in it, to receive the steel spindle KL, and at *f* there is just such another piece of brass foldered to the brass hoop *gb*, which furrounds both legs of the instrument. There is a small shoulder on the spindle at *f*, upon which the instrument

rests,

B b b 2

rests, and a small nut at  $Y$ , to prevent it from being blown off the spindle by the wind. The whole instrument is easily turned round upon the spindle by the wind, so as always to present the mouth of the kneed tube towards it. The end of the spindle has a screw on it; by which it may be screwed into the top of a post, or a stand made on purpose. It also has a hole at  $L$ , to admit a small lever for screwing it into wood with more readiness and facility. A thin plate of brass  $k$  is foldered to the kneed tube, about half an inch above the round hole  $G$ , so as to prevent rain from falling into it. There is likewise a crooked tube  $AB$  (fig. 2.), to be put on occasionally upon the mouth of the kneed tube  $F$ , in order to prevent rain from being blown into the mouth of the wind-gage, when it is left out all night, or exposed in the time of rain. The force or *momentum* of the wind may be ascertained by the assistance of this instrument, by filling the tubes half-full of water, and pushing the scale a little up or down, till the  $o$  of the scale, when the instrument is held up perpendicularly, be on a line with the surface of the water, in both legs of the wind-gage. The instrument being thus adjusted, hold it up perpendicularly, and turning the mouth of the kneed tube towards the wind, observe how much the water is depressed by it in the one leg, and how much it is raised in the other. The sum of the two is the height of a column of water which the wind is capable of sustaining at that time; and every body that is opposed to that wind, will be pressed upon by a force equal to the weight of a column

lumn of water, having its base equal to the surface that is opposed, and its height equal to the altitude of the column of water sustained by the wind in the wind-gage. Hence the force of the wind upon any body where the surface opposed to it is known, may be easily found; and a ready comparison may be made betwixt the strength of one gale of wind and that of another, by knowing the heights of the columns of water, which the different winds were capable of sustaining. The heights of the columns in each leg will be equal, provided the legs are of equal bores; but unequal, if their bores are unequal. For suppose the legs equal, and the column of water the wind sustains to be three inches, the water in the leg, which the wind blows into, will be depressed one inch and a half below  $\phi$ , and raised just as much above it in the other leg. But if the bore of the leg which the wind blows into, be double that of the other, the water in that leg will be depressed only one inch, whilst it is raised twice as much, or two inches, in the other; and *vice versa*, if the same wind blow into the smaller leg, it will depress the water in it two inches, whilst it raises it only one inch in the other. The force of the wind may be likewise measured with this instrument, by filling it until the water runs out at the hole  $\phi$ . For if we then hold it up to the wind as before, a quantity of water will be blown out; and, if both legs of the instrument are of the same bore, the height of the column sustained, will be equal to double the column of water in either leg, or the sum of what is wanting in both legs.

But

But if the legs are of unequal bores, neither of these will give the true height of the column of water which the wind sustained. But the true height may be obtained by the following *formule*.

Suppose that after a gale of wind, which had blown the water in one of the tubes from A to B (fig. 3.), forcing it at the same time through the other tube out at E, the surface of the water should be found standing at some level DG, and it were required to know what was the height of the column EF or AB, which the wind sustained. In order to obtain which, it is only necessary to find the height of the columns DB or GF, which are constantly equal to each other: for either of these added to one of the equal columns AD, EG, will give the true height of the column of water which the wind sustained.

#### C A S E I.

Let the diameters AC, EH, of the tubes be respectively represented by  $c$ ,  $d$ ; and let  $a$ =AD or EG, and  $x$ =DB or GF. Then it is evident, that the column DB is to the column EG as  $c^2x$  to  $d^2a$ . But these columns are equal. Therefore,  $c^2x=d^2a$ ; and consequently,  $x=\frac{d^2a}{c^2}$ .

#### E X A M P L E.

If the diameters AC, EH, be respectively 10 and 1, and AD or EG=3,96 inches,  $x$  will be=.0396 of an inch. For  $d^2a=1 \times 3,96=3,96$ , which divided by  $c^2=100$ , gives  $x=.0396$ .

C A S E II.

But if at any instant of time, whilst the wind was blowing, it was observed, that when the water stood at E, the top of the tube out of which it is forced, it was depressed in the other tube to some given level BF, the altitude at which it would have stood in each, had it immediately subsided, may be found in the following manner:

Let  $b = AB$  or  $EF$ . Then it is evident, that the column DB is equal to the difference of the columns EF, GF. But the difference of these columns is as  $d^2b - d^2x$ . Therefore  $c^2x = d^2b - d^2x$ ; and consequently,  $x = \frac{d^2b}{c^2 + d^2}$ .

For the cases when the wind blows in at the narrow leg of the instrument.

Let  $AB = EF = b$ ,  $EG$  or  $AD = a$ ,  $GF = DB = x$ , and the diameters EH, CA, respectively  $= d, c$ , as before. Then it is evident, that the column AD is to the column GF as  $ac^2$  to  $d^2x$ . But these columns are equal. Therefore,  $d^2x = ac^2$ ; and consequently,  $x = \frac{ac^2}{d^2}$ . This answers to

CASE I.

It is also evident, that the column AD is equal to the difference of the columns AB, DB. But the difference of these columns is as  $bc^2 - c^2x$ . Therefore,  $d^2x = bc^2 - c^2x$ .

Whence we get  $x = \frac{bc^2}{d^2 + c^2}$ . This corresponds to CASE II.

As there is always a calculation to be made for every

experiment when the legs of the instrument are of unequal bores, I would recommend it to the makers of these instruments, to make use of tubes that are equal, or at least nearly so, that the error may become next to nothing, it being a thing very easy to be done. In this manner we can readily determine the greatest force, which the wind has blown with, during the time the instrument has been exposed to its action. But as it may be safely left alone, by screwing its spindle into the proper stand, or into the top of a post, and as the wind never fails to turn the mouth of it towards itself, it is not necessary for the observer to continue always by it; for it may be allowed to stand all night, exposed to the wind, without any inconvenience, though it should even happen to rain very heavily. However, recourse can only be had to this method of using the instrument on shore: for at sea it must always be held up in a perpendicular position in the hand, whether it be used when only half full of water, or when quite full; which last will be frequently found to be the only practicable method of ascertaining the force of the wind during the night, when it blows so hard that it is impossible to keep any lights on deck. A person filling the wind-gage, in a calm place, with water, in order to determine the force of the wind, in the way which I have been just now describing, will be apt to imagine, that it cannot give the measurement correct; for he will find such a repulsion to arise from the edges of the hole *g*, as to sustain a column of water in the kneed or bent tube, perhaps half an inch above the level;

level: but by either blowing across the round hole, or moving his finger over it, he will soon bring the water in the kneed tube to stand at the same level with it, by taking off gradually the convex surface of the water, which projects out at the hole in the form of a drop or *spherule*. And this effect the wind very soon produces itself. There ought always to be a cover on the top of the tube out of which the water is expelled by the wind; but it should be made very thin. For if there be no such cover, and the mouth of the kneed tube be stopped, after the instrument is quite full of water, in order to prevent the wind from having any influence in raising it, you will find, upon exposing it to a strong gale, that in a very short time it will blow out perhaps half an inch of water. Whence it appears, that a very considerable error would arise from using the wind-gage in this state. But in all the experiments which I have made with this instrument, whilst it had the cover and the round hole of  $\frac{2}{10}$ ths of an inch in diameter in the middle of it, I have not been able to discover any error. The use of the small tube of communication *ab* (fig. 1.) is to check the undulation of the water, so that the height of it may be read off from the scale with ease and certainty. But it is particularly designed, to prevent the water from being thrown up to a much greater or less altitude, than the true height of the column, which the wind is able at that time to sustain, from its receiving a sudden impulse, whilst it is vibrating either in its ascent or descent. For water in the legs of a siphon is capable of being put

VOL. LXV. C c c into



into a vibrating motion like a pendulum<sup>(a)</sup>; and therefore, if acted upon when in the ascent, the height which it ascends to will come out greater than the truth; and less, if acted on in the descent.

The height of the column of water sustained in the wind-gage being given, the force of the wind upon a foot square is easily had by the following table, and consequently on any known surface.

T A B L E I.

Height of the water in the wind-gage.	Force of the wind on the foot square in avoirdupois pounds.	Common designation of such a wind.
12 inches	62,5	} most violent hurricane.
11	57,293	
10	52,083	
9	46,875	
8	41,667	
7	36,548	very great ditto.
6	31,75	great hurricane.
5	26,041	hurricane.
4	20,833	very great storm.
3	15,625	great ditto.
2	10,416	storm.
1	5,208	very high wind.
$0\frac{1}{2}$	2,604	high wind.
$0\frac{1}{10}$	,521	brisk gale.
$0\frac{1}{20}$	,260	fresh breeze.
$0\frac{1}{40}$	,130	pleasant wind.
		a gentle wind.

(a) NEWTONI Princip. Mathematic. lib. II. prop. XLIV. theor. XXXV.

E X A M P L E.

If it were required to know the force of the wind, when the column of water sustained was equal to  $4\frac{6}{7}$  inches. Then, by TAB. I.

	Pounds.
4 inches =	20,833
0,5 or $\frac{1}{2}$ inch =	2,604
0,1 =	0,521
<hr/> Sum $4,6 =$	<hr/> 23,958 = force on every square foot.

Any change that can happen in the specific gravity of the water from heat or cold, will make no sensible alteration on experiments made with this instrument.

A cubic foot of water is generally supposed to weigh 1000 Avoirdupois ounces; and from some experiments made by Mr. MUSSCHENBROEK it would appear, that betwixt freezing and boiling, or in  $180^{\circ}$  on FAHRENHEIT'S scale, it increases only  $\frac{1}{85} = ,0117$  of its whole bulk, or volume<sup>(b)</sup>. I cannot, however, find any author that mentions at what precise degree of heat a cubic foot of water was weighed. Mr. FAHRENHEIT indeed made several of his curious experiments on the specific gravities of bodies when the water raised his thermometer to  $48^{\circ}$ <sup>(c)</sup>. Now if we suppose the greatest heat of the water which

(b) MUSSCHEN. Introd. ad Philos. Natur. tom. II. p. 625.

(c) Philosophical Transactions, N<sup>o</sup> 383.

we make use of in the wind-gage to be  $90^{\circ}$ , which exceeds  $48^{\circ}$  by 42, the greatest change produced will be only ,0027 or  $\frac{27}{10000}$  parts of the whole. So that if the altitude of the column of water sustained by the wind were even to be five inches, the part of this effect, arising from the diminution of the specific gravity of the water, occasioned by the greatest heat, will only amount to 0,0135, or  $\frac{135}{10000}$  parts of an inch, a change which cannot be measured by the instrument. It may be sometimes necessary to employ other fluids besides water, particularly if the degree of cold be below freezing: for then we must use a fluid that will not freeze in the degree of cold in which we expose the instrument, otherwise the wind can have no influence on it, and the liquor freezing in the tube will break it. I shall, therefore, mention a few liquors in the following table that will answer the purpose, as also subjoin a general method of reducing them all to one common measure. But of all the fluids I am acquainted with, when the effects of frost are to be feared, I know none better adapted to our purpose than a saturated solution of sea-salt; since it does not freeze till the thermometer falls to 0 degrees, and is a fluid constantly of the same specific gravity. Spirit of wine, independent of its being more variable in respect of specific gravity by the influence of heat and cold, is also more or less so, as it is more or less rectified. And although the true specific gravity were known at the beginning of the operation, it would even change during the time of using it, by imbibing moisture from the air.

Let  $w$  represent the weight of a column of water, having its altitude measured by one of the divisions on the scale, and its base to any given surface whatever; and let  $n$  denote in general the number of these divisions which measures the whole length of the column of the water which the wind sustains. Then  $nw$  will represent always its weight, and will serve as a common multiplier for the specific gravities of all other liquors.

T A B L E II.

Names of liquors.	Specific gravities.	Common multiplier.	Weight measuring the forces of the wind.
Water,	1,000	} $nw$	$nw$
Sat sol. of salt,	1,244		$1,244 \times nw$
Urine,	1,030		$1,030 \times nw$
Ditto,	1,016		$1,016 \times nw$
Alcohol,	0,825		$0,825 \times nw$
Proof spirits,	0,927		$0,927 \times nw$
&c. &c.			&c. &c.

E X A M P L E.

Let  $w$  represent the weight of a column of water  $\frac{1}{20}$ th of an inch high, standing on a square foot; and let  $n=80=4$  inches. Then (by TAB. I.)  $nw$  is equal to 20,833 Avoirdupois pounds. Therefore  $1,244 \times 20,833$  = weight of a saturated solution of sea salt of the same altitude. and  $\frac{4}{1,244}$  = the altitude of a column of a saturated solution of the same, weighing 20,833 pounds Avoirdupois.

dupois.  $w$  may represent a square yard, the surface of a sail, &c.

If the velocity and density of the wind in any particular case were accurately determined, this instrument, which gives its force or *MOMENTUM*, would enable us to ascertain the velocity in every other case, the density being known. For it appears from experiments, made by Mr. JAMES FERGUSON, F. R. S. on the whirling-table, that its force is as the square of its velocity. But as the density, which is one of the *data* requisite for determining the velocity by this instrument, was not taken into consideration in these experiments, all that we can do at present is to suggest the idea.

It may not, perhaps, be improper to take notice, that evaporation will have some effect in diminishing the altitude of the column of water; though its influence, for the most part, will be very inconsiderable. The more frequently, therefore, the instrument is examined, it will be so much the better. If it be exposed to the action of the wind, whilst it happens to snow, it will be necessary to look at it frequently, lest the snow should choak up the mouth of the wind-gage.

Extract of a letter from Dr. LIND to Col. ROY. Dated  
Edinburgh, May 26, 1775.

It ought to be somewhat longer than that I lately sent Sir JOHN PRINGLE. For we had a gale here on the 9th current, which supported a column of water  
of





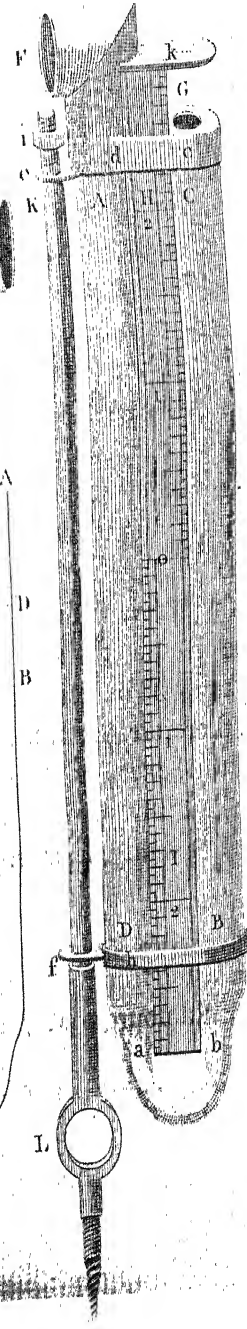


Fig. 1.





of  $6\frac{7}{10}$  inches, whereas that I sent was not so long. The force of this gale on a square foot was equal to 34,921 pounds Avoirdupois, and it has done great damage to our gardens. West India hurricanes would require gages of a still greater length to measure them.

XXXV. *Astronomical Observations made at Leicester. By the Reverend Mr. Ludlam, Vicar of Norton, near Leicester. Communicated by the Astronomer Royal.*

Redde, May 11, 1775.

OBSERVATIONS FOR DETERMINING THE LATITUDE OF  
THE PLACE.

Zenith distances taken with an eighteen inch quadrant,  
made by BIRD.

1774.	$\beta$ Draconis on quadrantal arch		$\gamma$ Draconis on quadrantal arch.	
	Degree s. M. S.	Parts. S. V.	Degrees. D. M. S.	Parts of 96. P. S. V.
July 2	9 32	1 5 $\frac{1}{2}$	1 6 40	1 1 7 $\frac{3}{8}$
10	9 20	1 5 $\frac{1}{4}$	1 6 34	1 1 7 $\frac{1}{8}$
14	9 20	1 5 $\frac{1}{2}$	1 6 34	1 1 7 $\frac{1}{2}$
15	9 20	1 5 $\frac{1}{8}$	1 6 28	1 1 7 $\frac{1}{4}$
20	9 20	1 5 $\frac{1}{8}$	1 6 27	1 1 7 $\frac{1}{8}$
Mean	9 22,4	1 5 $\frac{1}{16}$	1 6 32,6	1 1 7 $\frac{1}{16}$
	On arch of excess.		On arch of excess.	
	M. S.	S. V.	D. M. S.	P. S. V.
June 30	8 58	1 4 $\frac{1}{4}$	1 6 12	1 1 6 $\frac{1}{2}$
July 4	8 50	1 4 $\frac{1}{8}$	1 6 6	1 1 6 $\frac{1}{2}$
9	8 58	1 4 $\frac{1}{8}$	1 6 4	1 1 6 $\frac{1}{2}$
16	8 58	1 4 $\frac{1}{8}$	1 6 6	1 1 6 $\frac{1}{2}$
18	8 58	1 4 $\frac{1}{8}$	1 6 6	1 1 6 $\frac{1}{2}$
Mean	8 56,4	1 4 $\frac{1}{8}$	1 6 6,8	1 1 6 $\frac{1}{8}$

N.B. The seconds were shewn by the micrometer screw, the fractional parts estimated by the eye.

Reduce

Reduce the parts of 96 to degrees, and take the mean between the zenith distances shewn on each scale, and the zenith distance of  $\beta$  Draconis on the quadrantal arch will be  $9^{\circ} 21', 7''$ , and on the arch of excess  $8^{\circ} 54'$ , whence the true zenith distance is  $9^{\circ} 7', 8''$ , and the error of the line of collimation  $13', 8''$ , to be subtracted from the numbers shewn on the limb of the quadrant. In like manner we shall find the true zenith distance of  $\gamma$  Draconis  $1^{\circ} 6' 19', 8''$ , and the error of the line of collimation  $13', 5''$ . If we suppose the apparent declination of  $\beta$  Draconis on July 12th to be  $52^{\circ} 28' 52'', 3$ , that of  $\gamma$  Draconis  $51^{\circ} 31' 41'', 7$ , we have the latitude from the former  $52^{\circ} 38'$ , and from the latter  $52^{\circ} 38' 1''$ .

N. B. Some observations on these two stars in July 1772, give the same latitude within less than  $2''$ , but make the error of the line of collimation  $23''$  to be subtracted. I suspect the line of collimation is liable to small variations in portable quadrants, if not in all.

Zenith distances of  $\alpha$  Herculis with the state of the  
barometer and thermometer.

1774.	Degrees.	Parts of 96.	Barom.	Therm.
	D. M. S.	P. S. V.	Inches.	Degrees.
June 30	37 57 36	40 3 14 $\frac{1}{2}$	29,7	58
July 2	37 57 46	40 3 14 $\frac{3}{4}$	29,8	65
4	37 57 40	40 3 14 $\frac{1}{2}$	29,7	56
9	37 57 41	40 3 14 $\frac{2}{3}$	29,7	55
10	37 57 36	40 3 14 $\frac{1}{3}$	29,7	55
16	37 57 36	40 3 14 $\frac{1}{3}$	30,0	65
18	37 57 32	40 3 14 $\frac{1}{3}$	30,0	56
20	37 57 32	40 3 14 $\frac{1}{3}$	29,8	58
Mean	37 57 37,4	40 3 14 $\frac{1}{4}$	29,8	58,5

The mean of the zenith distances shewn on the two  
scales of divisions is  $37^{\circ} 57' 32''$ . Add for refraction  $43,4''$ :  
subtract for line of collimation  $13,6''$ : and we have the  
true zenith distance  $37^{\circ} 58' 1,8''$ . Suppose the apparent de-  
clination of  $\alpha$  Herculis on July 12th to be  $14^{\circ} 39' 58,5''$ ,  
we have the latitude  $52^{\circ} 38'$ .

# Zenith distances of the Pole-Star.

1774.	Degrees.	Parts of 96.	Barom.	Therm.
	D. M. S.	P. S. V.	Inches.	Degrees.
Nov. 10	35 27 56	37 6 9 $\frac{7}{8}$	30,05	31
11	35 27 55	37 6 9 $\frac{1}{2}$	29,87	32
13	35 27 56	37 6 9 $\frac{7}{8}$	30,20	34
Mean	35 27 55,6	37 6 9 $\frac{3}{4}$	30,04	32,3
Dec. 6	35 28 8	37 6 10 $\frac{1}{8}$	30,27	30
15	35 28 4	37 6 10 $\frac{1}{4}$	30,03	44
Mean	35 28 6	37 6 10 $\frac{3}{8}$	30,15	37
Dec. 12	39 15 0	41 6 14 $\frac{3}{4}$	29,83	44
13	39 15 4	41 6 15	28,86	44
15	39 15 0	41 6 14 $\frac{1}{2}$	30,08	38
Mean	39 15 1,3	41 6 14 $\frac{1}{6}$	29,59	42

Take the mean between the two scales of divisions, and we have the mean zenith distances, as follows:

Days of the month.	Obs. zenith dist.	Cleared of refr.
	D. M. S.	D. M. S.
Nov. 10 11 13	35 27 49,5	35 28 33,5
Dec. 6 15	35 28 0,3	35 28 44,
Dec. 12 13 15	39 14 59,3	39 15 46,7

The mean zenith distance of Nov. 10. 11. 13. cleared of refraction, is 35° 28' 33,5". To this add 7,5" for the increase of apparent declination between Nov. 12. and Dec. 12. and we have the zenith distance on Dec. 12. (as derived from the observations in November) 35° 28' 41".

D d d 2

The

The same from the actual observations on Dec. 6. and 15. is  $35^{\circ} 28' 44''$ . The mean of these two, corrected for the line of collimation, gives the true zenith distance above the pole,  $35^{\circ} 28' 28,9$ . The observations of Dec. 12. 13. 15. cleared of refraction and corrected for the line of collimation, give the true zenith distance below the pole,  $39^{\circ} 15' 33,1$ , whence the latitude  $52^{\circ} 37' 59$ , and the apparent declination of the pole star, Dec. 12th,  $88^{\circ} 6' 27,9$ . From all these observations we may conclude the latitude of (St. Martin's church in) Leicester is  $52^{\circ} 38'$ , within very few seconds<sup>(a)</sup>. From some observations made with an HADLEY'S quadrant of six inches radius, and given in the Transactions for 1769, I made the latitude only  $52^{\circ} 37' 3''$ ; but those observations cannot be set in competition with these, either for weight or number.

Occultations of  $\gamma$  and  $\alpha$  Tauri, observed at Leicester,  
Nov. 18, 1774.

	Time by the clock.
Emerſion $\gamma$ Tauri,	H. M. S. VI 27 10.
Immerſion $\alpha$ Tauri. Touched the limb,	XIV 59 26
Vanished,	XIV 59 30
Emerſion $\alpha$ Tauri inſtantaneous,	XVI 12 38

(a) The observations were made in Wigſton's hoſpital adjoining to the church.

The following observations serve to examine the clock.

Transits of the Sun.				
Day of the Month.	1st wire.	Time of the Clock.		3d wire.
		Meridian.		
1774.	M. S.	H.	M. S.	M. S.
Nov. 17	43 35	XXIII	44 24½	45 11¾
	45 54		46 43	47 30
18	43 50	XXIII	44 39	45 26
	46 8½		46 57	47 44

Hence the *rate* of going was conformable to mean time.

Zenith distances taken with the eighteen inch quadrant, to ascertain the absolute error of the clock, Nov. 18.  
Barometer 29,6 inches. Thermometer 33°.

Time by clock.		Degrees.	Parts of 96.		Object.
H.	M. S.	D. M.	P.	S. V.	
VIII.	0 0.	67 11½	71	5 6	} α Aquilæ.
	2 46	67 35½	72	0 13½	
	12 15	68 59	73	4 11	} α Aquilæ.
	14 37	69 20	73	7 10½	
	48 4	56 45¾	60	4 5½	} α Tauri.
	50 55	56 20½	60	0 13½	
	53 23	55 59½	59	5 13	} β Geminorum.
XI	1 57	55 21	59	0 5	
	3 54	55 3	58	5 12	
	6 35	54 39	58	2 5	} α Tauri.
XVI	26 33	58 45¾	62	5 7	
	29 49	59 14½	63	1 8	
	33 9	59 44	63	5 12	
	36 15	60 11½	64	1 10	

From



From the mean of the two first zenith distances of  $\alpha$  Aquilæ the clock will be found to be slower than mean time 14". By the second pair of  $\alpha$  Aquilæ, 13". By the next three zenith distances of  $\alpha$  Tauri, 13". By the next three of  $\beta$  Geminorum, 16". By the last four of  $\alpha$  Tauri, 17"; the error of the line of collimation being 13,6 as before. The mean of all these gives the clock 14,6 slower than mean time. Hence,

	Solar time.
	<hr/> H. M. S. <hr/>
Emerfion of $\gamma$ Tauri,	vi 41 48,4
Immerfion $\alpha$ Tauri. Touched limb,	xv 13 59,6
Vanifhed,	xv 14 3,6
Emerfion $\alpha$ Tauri, instantaneous,	xvi 27 10,8

The emerfion of  $\alpha$  Tauri was obferved at Greenwich at xvi 34 36,8 folar time.

N. B. The immerfion of  $\zeta$  Tauri (behind the Moon) which was obferved at Leicefter, April 28, 1770, at ix 45 44 folar time, was alfo obferved at Greenwich at ix 51 28,6 folar time. See Phil. Tranf. for 1770.

XXXVI. *Remarks and Considerations relative to the Performance of Amputation above the Knee, by the single circular Incision.* By Benjamin Gooch, Surgeon at Norwich.

Redde, May 19, 1775. **I**N April, 1774, I was asked, in consultation with several other surgeons, in the case of a bad leg, in a lad of about fifteen years of age, belonging to the Belchamp-house of Industry, in the county of Suffolk. The propriety of the operation above the knee was indisputable; and the single, instead of the double, incision, which I proposed to the consideration of my brethren, was unanimously assented to, and done in the following manner.

After drawing up the teguments as far as could be done, fixing them tight and very exactly with a narrow band<sup>(a)</sup>, before straitening the tourniquet ligature, the operation was performed by Mr. REVANS, of Halesworth, who made the incision quick and with great exactness, quite round to the bone, steadily at once; rather lower posteriorly and internally than anteriorly and externally, as the

(a) The band may be about an inch broad, made of firm linen cloth or thin leather, spread with common plaster; and in order to increase its power, it may have a slit in the middle, and be used like the uniting bandage.

band directed, in consideration of the different attachment of the muscles. These were then carefully dissected from the *periosteum*, and drawn up with the retractor, as I formerly described; so that after the use of the saw, the end of the bone was lodged full two inches in the muscles<sup>(b)</sup>. This manner of operation succeeded to our wish; leaving a flat, even stump, which was completely cicatrized in less than two months; and with but little exfoliation from the bones, as Mr. RE-VANS informed me: since that, he assured me, that it was the best stump he had ever seen, which he ascribed to the manner of amputating, and the treatment afterwards.

The following method in this case was used to keep down the teguments and muscles, during the cure of the stump, which I have practised more than forty years, much to my own satisfaction, and the approbation of many of my brethren. As soon as the tension is over, and the wound well digested, I apply a circular plaster, moderately adhesive, four or five inches broad, with great exactness, near to the edge of the wound; and then

(b) See Chapter upon Amputations in my Cases and Remarks in Surgery, edit. 2.; and what M. LOUIS says upon this subject, in the *Mém. de l'Acad. Roy. de Chirurgie*, tom. II. et IV. In the latter of which, this eminent surgeon mentions his having had compression successfully made upon the crural artery in the groin, in some amputations that he performed, where there was not room to apply the tourniquet to the thigh. The celebrated M. PETIT, many years ago, preserved the life of a gentleman, by inventing a machine to serve such a purpose, on account of a hæmorrhage, twenty-one days after having had his thigh amputated higher up than ordinary.

affix to the whole breadth of it, at due distances, six straight straps of the same kind of plaster, of suitable dimensions, with flits in half of them, for their opposites to be passed through over the necessary dressings; and an easy well-adapted compress of a sufficient thickness, having a piece of stiff paper on the outside of it; directing an assistant to thrust gently and evenly down, with both hands, the teguments and muscles, whilst I draw the straps properly strait, and secure the ends of them when reflected, with a narrower circular plaster. All this can be easily removed, and occasionally renewed at the time of dressing, letting the other circular plaster remain without renewal, as long as its adhesion effectually answers the purpose. And after proceeding in this manner, I generally find a cap sufficient, without any bandage (c).

This is not the only instance in which I have had good reason to approve the single incision; and should it be found, by sufficient experience, to answer the end in general, but as well as the double, in preventing a conical stump (d), as mentioned in my Cases and Remarks in Surgery, it will indisputably deserve the preference; and though, from what I have observed, there seldom is much projection of the bone attending the double incision,

(c) When I made some remarks upon amputation formerly, I should have advised this method, from its great utility experienced in many instances.

(d) I have been informed, by good authority, that a resection of the bone is no uncommon practice at Paris, to remedy this inconvenience.

when it has been properly made, and the cure of the wound judiciously conducted; yet I have sometimes observed, notwithstanding the utmost judgement and attention, some appearance of want of muscular substance to form a well-shaped stump. And in whatever method an amputation has been performed, a very strait bandage should by all means be avoided in the subsequent treatment of the wound, for obvious reasons; namely, because it obstructs circulation, and is an impediment to nutrition. The condition of the patient, the state of the limb, and the part of it where the operation is sometimes found absolutely necessary to be performed, with other concurring circumstances may prevent the formation of a good stump under the most skilful management, whether the single or double incision was practised, according as the surgeon shall find reason to determine his choice, by duly exercising his judgement in this nice point. Considering the different degrees of attachment and adhesion of the muscles, by means of the cellular membrane, if kept firmly together with the strait narrow band, as has been described, it is evident, they will be more evenly and regularly divided by the single than by the double incision; for when the muscles are laid bare, if a stimulus of any kind is applied to their fibres, it is evident, they will contract more or less, according to their different attachments, and different degrees of irritability: and for this reason an even section will not easily be effected, unless the whole incision

fion

fion be made at once, and as it were in an instant, so as not to allow sufficient time for the irritation of the knife to take effect upon the bare muscles<sup>(e)</sup>.

(e) See two Dissertations, lately published by my ingenious friend Dr. KIRKLAND, of Ashby de la Zouch; the one upon the Brain and Nerves; and the other on the Sympathy of the Nerves, and Irritability. I am also inclined to think, and not without the authority of experience, that amputation below the knee, by the single incision, may be found preferable; observing what has been mentioned, and cutting the *gastrocnemius* muscle a little shorter than the other muscles, as Mr. SHARP directs in his Chirurgical Operations.

XXXVII. *Concerning Aneurysms in the Thigh.* By Benjamin Gooch, Surgeon at Norwich.

Redde, May 19, 1775. **I**N the case described in the preceding paper, we observed a division of the femoral artery into two trunks of equal size, running parallel, and so near together, as that we could conveniently include them in one ligature with the needle, avoiding the nerve, after raising them up with the dissecting forceps, by a small portion of the connecting cellular membrane. And here we found no occasion to take up any other vessel. This makes the third instance in amputations of the thigh, in which I have observed such a *usus naturæ* in the arterial system; hence I am inclined to think it not improbable that this has often happened, though I do not find it has been noticed by any other surgeon. The remark may prove of some practical use, as I have hinted in my Treatise on Wounds, p. 78. in respect to aneurysms in the thighs. It might indeed reasonably be feared, where there is only one trunk of the femoral artery without a division, which is commonly supposed to be the general case, that the lateral and communicating branches would not sufficiently enlarge, to carry on the circulation of the blood, and preserve the life and use of the limb after the operation for an aneurysm.

This point seems worthy of particular consideration; and indeed it proved the subject of some controversy, in the case of an aneurysm about four inches above the knee of a healthful young man (supposed to proceed from a fall a year before) in which my opinion was asked. Some months after, one of the surgeons concerned in the consultation told me, there appeared at this time no hopes of succeeding by the operation for the aneurysm; and but little of saving the patient's life by amputation. It is not to be expected that an operation of this nature, so very precarious in this part, should in every instance be attended with the success, which Mr. LESLIE, an eminent surgeon at Corke, had the satisfaction to experience, in a case, which is related in the Medical and Philosophical Commentaries of the Edinburgh Society, N° VI. p. 176. § 2. The surgeon, however, ought by no means to incur censure for the unfortunate event, after having taken all prudent and probable measures, to preserve his patient's limb<sup>(a)</sup>.

(a) The celebrated CYPRIANUS, who was professor of anatomy and surgery in the university of Francker, and the most famous lithotomist of his time, after successfully performing the Cæsarian operation at Lewarden, in which he found the *fœtus* in the right Fallopian tube, wrote a letter, upon that occasion, to Sir THOMAS MILLINGTON, physician to CHARLES II.; and afterwards he happily cut that eminent physician for the stone, at the age of sixty-eight, in London. In which letter, he earnestly admonishes surgeons not to be intimidated by threatening prospects, from undertaking dangerous and difficult operations, lest their reputations should suffer for want of success. This letter, written in French, consists of seventy pages, containing much solid, practical knowledge and was annexed to BELLOSTE's 4th edition of his *Chirurgien de l'Hôpital*, printed at Amsterdam in the year 1707.

From



From these considerations, I communicated my thoughts upon this subject to some of my brethren, wishing to have experiments made upon brutes, that might ascertain as far as possible, by analogy, a matter which appeared to us of great importance; and this motive induced one of them, with the assistance of a person of superior knowledge in the anatomy and diseases of horses, to resolve upon dissecting out of the thighs of a horse and a dog, the first opportunity that offered, the trunk of the artery to the length of two or three inches; to observe whether there was such a division of it, as I have remarked in the human species; and then to treat the wound as after the operation for an aneurysm, attending particularly to all the consequences. This kind of *lusus naturæ* has been often found in the *humerus*, by anatomists. After having engaged my friends at Norwich in this pursuit, something happening to prevent their carrying the design into execution so soon as they intended, determined me, with the assistance of Mr. RE-VANS, whom I have mentioned before, to make the experiment upon a full-grown young spaniel, and to keep a journal of the occurrences in consequence thereof.

On January the 19th, 1775, we performed the operation with very little trouble, having securely bound the animal to prevent interruption. We designedly included in the ligatures, with the trunk of the artery, a little above the middle of the thigh, the vein and nerve accompanying it, in order to render the experiment more decisive,

decisive, if it succeeded, than it would have been, had the artery alone been taken in. We discovered no division in the trunk of the artery like what I have observed; and having made the wound of a sufficient extent, we succeeded at once in passing the ligatures with the needle, without the least hæmorrhage. The wound was anointed with fresh hog's lard, to tempt the dog the more readily to lick it; and this application was often repeated, having at the same time the whole limb embrocated with *linimentum volatile*, which was well rubbed in. After the operation, the dog shewed no signs of great pain, had no spasmodic motions in the limb, but made no use of it, and we could feel no pulsation below the ligatures. On the 20th, nothing appeared worthy notice, only that there was a warmth throughout the limb, nearly to the same degree as in the other. 21st, A little œdematous swelling appeared upon the leg; in other respects, as yesterday. 22d, Same appearances as yesterday; begun to move the limb. 23d, Moved the limb rather more; wound looked well. 24th, Moved the limb more than yesterday; wound well digested; œdematous swelling less. 25th, The upper ligature came off; no hæmorrhage ensued. 26th, Wound in a healing state; he began to step upon the limb; œdematous swelling quite dispersed. 27th, All appearances favourable. 28th, The other ligatures came off, without any oozing of blood. 29th, The wound contracted into a narrow compass, by virtue of his

his own balsamic tongue<sup>(b)</sup>; no other application was made to the wound or limb than mentioned at first. 30th, No material alteration. 31st, He used the limb almost as well as the other. Feb. 9th, Dr. D'URBAN, of Halefworth, who attended to the sequel of this experiment, strictly examined the limb with Mr. REVANS and me; and we could feel no pulsation of the trunk of the artery in the space, nor in the lower part of the limb, after the wound had been perfectly healed some days. The dog was then remarkably brisk and lively, and as active as usual, without any impediment in the motion of the limb; and no swelling remained in any part of it.

We kept the dog alive till the 25th of March, and examined the limb attentively from time to time, without finding any visible defect in it, or want of sensation. Then, it not being likely that any thing more remarkable should appear, while he lived, we had him killed for farther inquiry. We had an apparatus ready for injection; but upon consideration, that carefully dissecting out the parts where we had performed the operation, and then carefully examining them, might more effectually answer our intention, we omitted the use of it. We did not find the artery divided into two equal branches, as I have mentioned and described in the human subject; but a ramification evidently appeared to

(b) See M. BELLOSTE's Dissertation upon the healing Virtue of a Dog's Tongue, in vol. II. of his Hospital Surgeon. See also the note at p. 173. of my Cases and Remarks in Surgery, edit. 2.

us much enlarged, by what we observed in the other limb, which departed from the trunk at an acute angle, just above where we passed the upper ligature in the operation; and the space between the two ligatures was filled up with a fleshy substance. Dr. D'URBAN was also present at this examination.

XXXVIII. *An Account of further Discoveries in Air.* By  
*the Rev. Joseph Priestley, LL.D. F. R. S. in Letters to*  
*Sir John Pringle, Bart. P. R. S. and the Rev. Dr.*  
*Price, F. R. S.*

## L E T T E R I.

TO SIR JOHN PRINGLE, BART. P. R. S.

DEAR SIR,

March 15, 1775.

Redde, May 25,  
 1775.

**H**AVING been pretty fortunate in the  
 prosecution of my experiments on dif-  
 ferent kinds of air, since the publication of my treatise  
 on that subject, I think it due to the attention with which  
 you have from the first honoured them, to give you some  
 account of what I have lately done. I know that every  
 new discovery, in any branch of natural knowledge,  
 gives you pleasure; and it is peculiarly flattering to me,  
 that you consider some of those, which I have been happy  
 enough to make, in a light of some importance. As I  
 have materials enough for another separate publication, I  
 shall not trouble the Society with a particular account of  
 my observations; but if you think proper to communicate  
 to them the following very general account, as a mark  
 of my respect for the Society, as well as for yourself, you  
 will

will add to the many obligations you have conferred upon me.

To the marine acid air, which I had discovered at the time of my former publication, I have now added three more; *viz.* the vitriolic, the nitrous, and the vegetable. The vitriolic acid air is produced by boiling in oil of vitriol any inflammable matter, or almost any thing that contains *phlogiston*; as oil, camphor, spirit of wine, charcoal, and most of the metals. For though this acid seems to have no affinity with some of these substances when it is cold, it affects them considerably, and particularly takes *phlogiston* from them, when it is hot; and by means of the *phlogiston*, of which it deprives them, it is early made volatile, so as to assume the form of a transparent air, like that of the marine acid; being as readily imbibed by water, and as readily forming a white cloud upon the admission of alkaline air. But the affinities of the vitriolic acid air with various substances, and many of the phenomena attending it, are strikingly different from those of the marine acid air. I thought it a little singular, that the solution of iron, zinc, and tin, in a diluted vitriolic acid should yield inflammable air; and that when boiled in the same acid concentrated, they should chiefly yield acid air, which is not at all inflammable, and cannot be confined by water. This, however, is in fact the produce of the process; and the very same as when copper, silver, or quicksilver is boiled in the same acid. From gold, platina, or lead, I was not able to procure any air at all by this means.

The vegetable acid air is as easily procured from the concentrated vegetable acid, as the marine acid air is from spirit of salt; and, I think, in greater quantity. This air also is perfectly transparent, is instantly imbibed by water, and makes a white cloud upon the admission of alkaline air; though several of its properties are exceedingly different from those of the marine or vitriolic acid airs.

The nitrous acid I have exhibited in the form of air, though only, as it were, for a moment; since no fluid, that I am acquainted with, is capable of confining it. The more I consider the nitrous acid, the more wonderful and inexhaustible the subject appears. The kinds of air which it forms, according to its various combinations with *phlogiston*, are, I believe, more numerous than all the kinds that can be formed by the other acids. Many of the phænomena which have lately occurred to my observation relating to it are, to me, altogether inexplicable; though I perceive certain analogies among some of them. Upon this subject I shall have a pretty long chapter. But, to avoid being tedious at present, I shall only observe, that by boiling various hard substances containing *phlogiston*, and especially charcoal, in the nitrous acid, I get genuine nitrous air, the very same that I get from the solution of various metals in that acid. At the time of my last publication I had not a large burning lens; and as the focus of the mirror cannot be thrown upon any thing in the form of a powder, or that requires a solid support, my experiments with the solar rays were exceedingly incomplete. I have now procured one of  
twelve

twelve inches in diameter; and the use of it has more than answered my highest expectations. The manner in which I have used it, has been to throw the focus upon the several substances I wished to examine, either *in vacuo*, or when confined by quicksilver, in vessels filled with that fluid, and standing with their mouths immersed in it. I presently found that different substances yield very different kinds of air by this treatment; and though the reasons, or analogies, of the different products, in many of the cases, be sufficiently obvious; and such as I had conjectured *a priori*, yet in other cases I am not a little puzzled and surprized. Various metals yield inflammable air by this process; several saline substances yield fixed air; many metallic *calces* yield the same, and some of them a *phlogisticated* common air; and some of the precipitates, in which the nitrous acid was employed, yield nitrous air, in one or other of its forms. But the most remarkable of all the kinds of air that I have produced by this process is, one that is five or six times better than common air, for the purpose of respiration, inflammation, and, I believe, every other use of common atmospherical air. As I think I have sufficiently proved, that the fitness of air for respiration depends upon its capacity to receive the *phlogiston* exhaled from the lungs, this species may not improperly be called, *dephlogisticated air*. This species of air I first produced from *mercurius calcinatus per se*, then from the red precipitate of mercury, and now from red lead. The two former of the substances yield it pure;



pure; but the red lead I have generally met with yields a greater proportion of fixed air along with it. Another quantity, however, gave this air and hardly any thing else. On what this difference depends I cannot tell; but hope to be able to investigate. That this air is of that exalted nature, I first found by means of nitrous air, which I constantly apply as a test of the fitness of any kind of air for respiration, and which I believe to be a most accurate and infallible test for that purpose. Applying this test, I found, to my great surprize, that a quantity of this air required about five times as much nitrous air to saturate it, as common air requires. Common air is diminished about one-fifth, by a mixture of one-half nitrous air; but one quantity of this air was diminished one-half, and another two-thirds, by the addition of twice as much nitrous air; and three times the quantity, left it little more than it was at the first. A candle burned in this air with an amazing strength of flame; and a bit of red hot wood crackled and burned with a prodigious rapidity, exhibiting an appearance something like that of iron glowing with a white heat, and throwing out sparks in all directions. But to complete the proof of the superior quality of this air, I introduced a mouse into it; and in a quantity in which, had it been in common air, it would have died in about a quarter of an hour, it lived, at two different times, a whole hour, and was taken out quite vigorous; and the remaining air appeared to be still, by the test of nitrous air, as good as common air. This experiment I also repeated,

peated, and with nearly the same success, with another mouse, and another quantity of this air, the virtue of which had been impaired. Examining all the degrees of the calcination of lead, I found nothing but fixed air, or a little *phlogisticated* common air, till I came to *masficcot*, which is a state that precedes the red lead. This gave air about twice as good as common air, and the litharge, which follows the red lead, gave fixed again. Roman vitriol and sedative salt yielded air which was, as nearly as possible, of the same degree of purity with common air. My conjectures concerning the cause of these appearances are as yet too crude to lay before the Society. My present ideas of the last mentioned facts are, that, together with other observations which I shall lay before the publick, they afford some foundation for supposing, that the nitrous acid is the basis of common air, and that nitre is formed by a decomposition of the atmosphere. But it is possible I may think otherwise to-morrow. It is happy, when with a fertility of invention sufficient to raise *hypotheses*, a person is not apt to acquire too great attachment to them. By this means they lead to the discovery of new facts, and from a sufficient number of these the true theory of nature will easily result.

I have made many other original experiments of a miscellaneous nature; but I shall not take up your time with the mention of them in this place. If this imperfect communication gives you, or the Society, any satisfaction, I shall be very happy, and shall be encouraged to prosecute

prosecute these inquiries as much as my leisure from other pursuits will admit. I am, &c.

## L E T T E R II.

TO THE REV. DR. PRICE, F. R. S.

DEAR SIR,

April 1, 1775.

AS you are pleased to interest yourself in my experiments, I hope it will give you some pleasure to be informed, that, I think, I never was more successful than I have been in the few days that I have been able to attend to these matters, since my return into the country. By the heat of the flame of a candle, and catching the air that arises in the manner described in fig. VIII. pl. 2. in my late Treatise, I get the pure air I discovered in London in great plenty, from a variety of cheap materials; not only from red lead, but many earthy substances moistened with spirit of nitre and dried, as chalk and quicklime; which demonstrates that red lead, *mercurius calcinatus per se*, &c. extract the nitrous acid from the air; and that this acid is the most essential among the various ingredients which compose the atmosphere. From tobacco-pipe clay, and some other things, moistened with spirit of nitre, I get fixed air, which seems to prove that this species of air (which is a kind of acid) is a modification of the nitrous acid, and in some measure accounts for the existence

existence of so much fixed air in the atmosphere. I believe this experiment is the first instance of the proper generation of fixed air from other principles. What we have got of it hitherto has been by dislodging it from substances, that were supposed to contain it. Notwithstanding red lead yields so pure an air, paint made with it diminishes common air, and makes it noxious, as much as white paint; which seems to prove, that it is the oil, that yields the *phlogiston*, which injures the air to which it is exposed.

---

## L E T T E R III.

TO SIR JOHN PRINGLE, BART. P. R. S.

DEAR SIR,

London, May 25, 1775.

**A**S I am desirous to present to the Royal Society a general review of my late observations on air, without troubling them with a detail of my experiments, I beg you would lay before them the following particulars, in addition to those contained in the letter which I took the liberty to write to you, dated March 15, and in the extract from that to Dr. PRICE, dated April 1, 1775; submitting the whole to the disposal of the Society.

VOL. LXV.

G g

I have

I have found, that the earths of all denominations, even the crystalline and the talcky, which are thought to be insoluble in acids, yield a pure *dephlogisticated air*, when treated in the manner mentioned in my former letters; but that the calcareous earths, and some of the earths of metals, as red lead and the flowers of zinc, yield it in the greatest plenty. Upon the whole, I think, it may safely be concluded, that the purest air is that which contains the least *phlogiston*: that air is impure (by which I mean that it is unfit for respiration, and for the purpose of supporting flame) in proportion as it contains more of that principle; and that there is a regular gradation from *dephlogisticated air*, through common air, and *phlogisticated air*, down to nitrous air; the last species of air containing the most, and the first-mentioned the least *phlogiston* possible, the common basis of them all being the nitrous acid; so that all these kinds of air differ chiefly in the quantity of *phlogiston* they contain; though with respect to nitrous air, there seems to be a further difference in the mode of combination. By attending to the quantity of *phlogiston* contained in the substances with which the spirit of nitre is mixed, any of these kinds of air may be produced at pleasure, and sometimes all the kinds will be produced in the different stages of the same process. White wood-ashes yield an exceedingly pure air; but the least bit of charcoal in the ashes depraves the air; and if there be much charcoal in them, the whole produce will be strongly nitrous. The phenomena of detonation (which has been a very  
puzzling

puzzling appearance in chemistry) admit of a very easy explication by the help of my late experiments. It is generally supposed, that in this case a sulphur is formed, by the union of the nitrous acid and the *phlogiston* of the body with which it is detonated; which sulphur is so inflammable, that it cannot exist a moment without decomposition: and it has been thought, that in the process of making the *chylus* of nitre, the acid is intirely destroyed or changed. But, in both these cases, I have no doubt, that the acid enters into the composition of some of the kinds of air which are generated upon those occasions. I once mixed a quantity of the ore of lead with spirit of nitre, and when it was dry, put it into a gun-barrel, filled up to the mouth with sand, in order to collect the air that heat would expel from it, in the usual manner. The production of air was very great and rapid; and when the heat became considerable, all the contents of the gun-barrel were exploded with great violence, and a loud report, demolishing the vessel which I had placed to receive the air. The next time, putting the same materials into a glass vessel, and disposing the apparatus in such a manner as that the explosion could not affect the collected air, I found it to be very strongly nitrous. Such, therefore, I conclude to be the produce of the explosion of gun-powder, since charcoal with spirit of nitre yields this kind of air. In the detonation of nitre with substances that contain little *phlogiston*, the acid may form common air, or air much purer than that.

As I mean these letters to contain a general outline of what I have lately observed with respect to air, I shall add, that by the favour of that most intelligent and generous chemist Mr. WOLFE, I have lately procured some of that phosphoric spar, from which a new mineral acid, first discovered in Sweden, is procured. This acid, as well as the marine, vitriolic, and vegetable, I throw into the form of air, confined by quicksilver; and thus have an opportunity of examining its affinities with the greatest ease and certainty. I shall in this place only observe with respect to it, that this acid air decomposes nitre, but not near so rapidly as the marine acid air; and that the salt which is formed by its union with alkaline air is not sensibly soluble in water. I am, &c.

P. S. Upon second thoughts, I am not so well satisfied with my conjecture, hinted in the letter to Dr. PRICE, that fixed air may be a modification of the nitrous acid, though the experiment there mentioned seems to make it probable. But I lay no stress upon any opinions, farther than as they may lead to the discovery of new facts.

XXXIX. *An Account of the Gymnotus Electricus.*

By John Hunter, F. R. S.

Redde, May 11,

1775.

**T**O Mr. WALSH, the first discoverer of animal electricity, the learned will be indebted for whatever the following pages may contain, either curious or useful. The specimen of the animal which they describe was procured by that Gentleman, and at his request this dissection was performed; and this account of it is communicated.

This fish, on the first view, appears very much like an eel, from which resemblance it has most probably got its name; but it has none of the specific properties of that fish. This animal may be considered, both anatomically and physiologically, as divided into two parts; viz. the common animal part; and a part which is superadded, viz. the peculiar organ. I shall at present consider it only with respect to the last; as the first explains nothing relating to the other, nor any thing relating to the animal economy of fish in general. The first, or common animal part, is so contrived as to exceed what was necessary for itself, in order to give situation, nourishment, and most probably the peculiar property to the second. The last part, or peculiar organ, has an immediate connexion with the first; the body affording it a situation; the heart, nourishment; and the brain, nerves and probably its peculiar powers. For the first of these purposes, the body is extended out in length, being much longer than



than would be sufficient for what may be called its progressive motion. For the real body, or that part where the *viscera* and parts of generation lye, is situated, with respect to the head, as in other fish, and is extremely short; so that, according to the ordinary proportions, this should be a very short fish. Its great length, therefore, seems chiefly intended to afford a surface for the support of the peculiar organ: however, the tail-part is likewise adapted to the progressive motion of the whole, and to preserve the specific gravity; for the spine, *medulla spinalis*, muscles, fin, and air-bladder, are continued through its whole length. Besides which parts, there is a membrane passing from the spine to that fin which runs along the belly or lower edge of the animal. This membrane is broad at the end next to the head, terminating in a point at the tail. It is a support for the abdominal fin, gives a greater surface of support for the organ, and makes a partition between the organs of the two opposite sides.

#### DESCRIPTION OF THE ORGANS.

The organs which produce the peculiar effect of this fish, constitute nearly one-half of that part of the flesh in which they are placed, and perhaps make more than one-third of the whole animal. There are two pair of these organs, a larger, and a smaller; one being placed on each side. The large pair occupy the whole lower or anterior, and also the lateral part of the body, making the thickness of the fore or lower parts of the animal, and run almost through its whole length; viz. from the *abdomen* to near the end of the tail <sup>(a)</sup>. It is broadest on

(a) Vide pl. H. fig. I. KKK;

the sides of the fish at the anterior end, where it incloses more of the lateral parts of the body, becomes narrower towards the end of the tail, occupying less and less of the sides of the animal, till at last it ends almost in a point.

These two organs are separated from one another at the upper part, by the muscles of the back, which keep their posterior or upper edges at a considerable distance from one another<sup>(b)</sup>; below that, and towards the middle, they are separated by the air-bag<sup>(c)</sup>; and at their lower parts they are separated by the middle partition<sup>(d)</sup>. They begin forwards, by a pretty regular edge, almost at right angles with the longitudinal axis of the body, situated on the lower and lateral parts of the *abdomen*.

Their upper edge is a pretty straight line, with small indentations made by the nerves and blood vessels, which pass round it to the skin. At the anterior end they go as far towards the back as the middle line of the animal; but in their approach towards the tail they gradually leave that line, coming nearer to the lower surface of the animal. The general shape of the organ, on an external or side view, is broad at the end next to the head of the animal, becoming gradually narrower towards the tail, and ending there almost in a point. The other surfaces of the organ are fitted to the shape of the parts with which they come in contact; therefore on the upper and inner surface it is hollowed, to receive the muscles of the back<sup>(e)</sup>. There is also a longitudinal depression on its lower edge, where a substance

(b) Pl. IV. cccc.

(c) Pl. IV. D.

(d) Pl. IV. K.

(e) Pl. IV.

lies, which divides it from the small organ, and which gives a kind of fixed point for the lateral muscles of the fin (*a*). Its most internal surface is a plane adapted to the partition which divides the two organs from one another (*s*). The edge next to the muscles of the back is very thin, but the organ becomes thicker and thicker towards its middle, where it approaches the center of the animal. It becomes thinner again, towards the lower surface or belly; but that edge is not so thin as the other (*b*). Its union with the parts to which it is attached is in general by a loose, but pretty strong, cellular membrane; except at the partition, to which it is joined so close as to be almost inseparable.

The small organ lies along the lower edge of the animal, nearly to the same extent as the other (*a*). Its situation is marked externally by the muscles which move the fin under which it lies. Its anterior end begins nearly in the same line with the large organ, and just where the fin begins. It terminates almost insensibly near the end of the tail, where the large organ also terminates. It is of a triangular figure, adapting itself to the part in which it lies (*a*). Its anterior end is the narrowest part; towards the tail it becomes broader; in the middle of the organ it is thickest; and from thence becomes gradually thinner to the tail, where it is very thin. The two small organs are separated from one another by the middle muscles, and by the bones upon which

(*f*) Pl. IV. (*g*) Pl. IV. (*b*) Pl. IV. (*i*) Pl. II. fig. 1. LLL.  
(*h*) Pl. IV. II.

the bones of the fins are articulated (*m*). The large and the small organ on each side, are separated from one another by a membrane, the inner edge of which is attached to the middle partition, and its outer edge is lost on the skin of the animal (*n*). To expose the large organ to view, nothing more is necessary than to remove the skin, which adheres to it by a loose cellular membrane. But to expose the small organ, it is necessary to remove the long row of small muscles which move the fin.

#### OF THE STRUCTURE OF THESE ORGANS.

The structure is extremely simple and regular, consisting of two parts; *viz.* flat partitions or *septa*, and cross divisions between them. The outer edge of these *septa* appear externally in parallel lines nearly in the direction of the longitudinal axis of the body (*1*). These *septa* are thin membranes, placed nearly parallel to one another. Their lengths are nearly in the direction of the long axis, and their breadth is nearly the semi-diameter of the body of the animal (*2*). They are of different lengths, some being as long as the whole organ. I shall describe them as beginning principally at the anterior end of the organ, although a few begin along the upper edge; and the whole, passing towards the tail, gradually terminate on the lower surface of the organ; the lowermost at their origin terminating soonest. Their breadths differ in different parts of the organ. They are in general broadest near the anterior end, answering to the thickest part of

(1) Pl. III. E. (m) Pl. III. P. (n) Pl. II. KKK. (o) Pl. IV. HH.

the organ, and become gradually narrower towards the tail; however, they are very narrow at their beginnings or anterior ends. Those nearest to the muscles of the back are the broadest, owing to their curved or oblique situation upon these muscles, and grow gradually narrower towards the lower part, which is in a great measure owing to their becoming more transverse, and also to the organ becoming thinner at that place (*p*). They have an outer and an inner edge. The outer is attached to the skin of the animal, to the lateral muscles of the fin, and to the membrane which divides the great organ from the small; and the whole of their inner edges are fixed to the middle partition formerly described, also to the air-bladder, and three or four terminate on that surface which inclose the muscles of the back (*q*). These *septa* are at the greatest distance from one another at their exterior edges near the skin, to which they are united; and as they pass from the skin towards their inner attachments they approach one another (*r*). Sometimes we find two uniting into one. On that side next to the muscles of the back, they are hollow from edge to edge, answering to the shape of those muscles; but become less and less so towards the middle of the organ; and from that towards the lower part of the organ, they become curved in the other direction (*s*). At the anterior part of the large organ, where it is nearly of an equal breadth, they run pretty parallel to one another, and also pretty straight;

(*p*) Pl. IV. where the different breadths are seen in one view.

(*q*) Pl. IV.

(*r*) Pl. IV.

(*s*) Pl. IV.

but where the organ becomes narrower, it may be observed in some places, that two join or unite into one; especially where a nerve passes across. The termina-

tion of this organ at the tail is so very small that I could not determine, whether it consisted of one *septum* or more. The distances between these *septa* will differ

in fishes of different sizes. In a fish of two feet four inches in length, I found them  $\frac{1}{27}$  of an inch distant from one another; and the breadth of the whole organ, at the broadest part, about an inch and a quarter, in which space were thirty-four *septa*. The small or-

gan has the same kind of *septa*, in length passing from end to end of the organ, and in breadth passing quite across; they run somewhat serpentine, not exactly in straight lines. Their outer edges terminate on the

outer surface of the organ, which is in contact with the inner surface of the external muscle of the fin, and their inner edges are in contact with the centre-muscles.

They differ very much in breadth from one another; the broadest being equal to one side of the triangle, and the narrowest scarcely broader than the point or edge.

They are pretty nearly at equal distances from one another; but much nearer than those of the large organ, being only about  $\frac{1}{56}$ th part of an inch asunder: but they

are at a greater distance from one another towards the tail, in proportion to the increase of breadth of the organ. The organ is about half an inch in breadth, and has fourteen *septa*. These *septa*, in both organs, are very

tender in consistence, being easily torn. They appear to answer the same purpose with the columns in the *torpedo*, making walls or butments for the sub-divisions, and are to be considered as making so many distinct organs. These *septa* are intersected transversely by very thin plates or membranes, whose breadth is the distance between any two *septa*, and therefore of different breadths in different parts; broadest at that edge which is next to the skin; narrowest at that next to the center of the body, or to the middle partition which divides the two organs from one another. Their lengths are equal to the breadths of the *septa*, between which they are situated. There is a regular series of them continued from one end of any two *septa* to the other. They appear to be so close as even to touch. In an inch in length there are about 240, which multiplies the surface in the whole to a vast extent.

#### OF THE NERVES.

The nerves in this animal may be divided into two kinds; the first, appropriated to the general purposes of life; the second, for the management of this peculiar function, and very probably for its existence. They arise in general from the brain and *medulla spinalis*, as in other fish; but those from the *medulla* are much larger than in fish of equal size, and larger than is necessary for the common operations of life. The nerve which arises from the brain, and passes down the whole length of the animal (which I believe exists in all fish) is larger  
in

in this than in others of the same size, and passes nearer to the spine<sup>(u)</sup>. In the common eel it runs in the muscles of the back, about midway between the skin and spine. In the cod it passes immediately under the skin. From its being larger in this fish than in others of the same size, one might suspect, that it was intended for supplying the organ in some degree; but this seems not to be the case, as I was not able to trace any nerves going from it to join those from the *medulla spinalis*, which run to the organ. This nerve is as singular an appearance as any in this class of animals; for surely it must appear extraordinary, that a nerve should arise from the brain to be lost in common parts, while there is a *medulla spinalis* giving nerves to the same parts. It must still remain one of the inexplicable circumstances of the nervous system. The organ is supplied with nerves from the *medulla spinalis* from which they come out in pairs between all the *vertebræ* of the spine<sup>(x)</sup>. In their passage from the spine they give nerves to the muscles of the back, &c. They bend forwards and outwards upon the spine, between it and the muscles, and send out small nerves to the external surface, which join the skin near to the lateral lines. These ramify upon the skin, but are principally bent forwards between it and the organ, into which they send small branches as they pass along. They seem to be lost in these two parts. The trunks get upon the air-bladder, or rather dip between it and the muscles of the back, and continuing

(u) Pl. III. t.

(x) Pl. III. s.



their course forwards upon that bag, they dip in between it and the organ, where they divide into smaller branches; then they get upon the middle partition, on which they continue to divide into still smaller branches; after which they pass on, and get upon the small bones and muscles, which are the bases for the under fin, and at last they are lost on that fin. After having got between the organ and the above mentioned parts, they are constantly sending small nerves into the organs; first into the great organ, and then into the small one; also into the muscles, of the fin, and at last into the fin itself. These branches, which are sent into the organ as the trunk passes along, are so small, that I could not trace their ramifications in the organs. In this fish, as well as in the *torpedo*, the nerves which supply the organ are much larger than those bestowed on any other part for the purposes of sensation and action; but it appears to me, that the organ of the *torpedo* is supplied with much the largest proportion. If all the nerves which go to it were united together, they would make a vastly greater chord, than all those which go to the organ of this eel. Perhaps when experiments have been made upon this fish, equally accurate with those made upon the *torpedo*, the reason for this difference may be assigned.

#### B L O O D V E S S E L S.

How far this organ is vascular, I cannot positively determine; but from the quantities of small arteries going to it, I am inclined to believe, that it is not deficient in vessels.

vessels. The arteries arise from the large artery which passes down the spine; they go off in small branches like the *intercostals* in the human subject, pass round the air-bladder, and get upon the partition together with the nerves, and distribute their branches in the same manner. The veins take the same course backwards, and enter the large vein which runs parallel with the artery.

PLATE I. FIG. I.

Shews the whole animal of the full size. It lies on one side; which posture exposes the whole of the under fin. The head is twisted, to shew its upper part, on which are seen the eyes, &c.

FIG. II.

Shews the animal lying in the same position, but the head is twisted in the contrary direction, so as to expose its under surface. Between the two fins, and before the beginning of the under fin, is the cavity of the belly of the fish; at the anterior part of which cavity is the *anus*.

PLATE II. FIG. I.

Exhibits the whole of the two organs on each side, the skin being removed as far as these organs extend. A. The lower surface of the head of the animal. B. The cavity of the belly. C. The *anus*. D. The fin. E. The back of the fish where the skin has not been removed. FF. The fin which runs along the lower edge.

edge of the fish. GGG. The skin turned back. HHH. The lateral muscles of the above fin removed and carried back with the skin, to expose the small organ. I. Part of the muscle left in its place. KKK. The large organ. LLL. The small organ. MMMM. The substance which divides the large organ from the small. N. At this place the above substance is removed.

## P L A T E III. F I G. IV.

A section of the whole thickness of the fish near the upper part, a little magnified. The skin is removed as far back as the posterior edge of the organ, and the other parts immediately belonging to it, such as the *medulla spinalis*. There are several pieces or sections taken out of the organ, which expose every thing that has any relation to it. At the upper and lower ends of the figure, F F, the organ is entire, the skin only being removed. AA. The body of the animal near the back, covered by the skin. BB. The belly-fin, covered also by the skin. c. Part of the skin removed from the organ, and turned back. DD. The muscles which move the fin laterally, and which immediately cover the small organ. E. The middle muscles of the fin, which lay immediately between the two small organs. FF. The outer surface of the large organ, as it appears when the skin is removed. G. The small organ, as it appears when the lateral muscles are removed. HH. The cut ends of the muscles of the back, which have been removed to expose the deeper seated





Fig 5

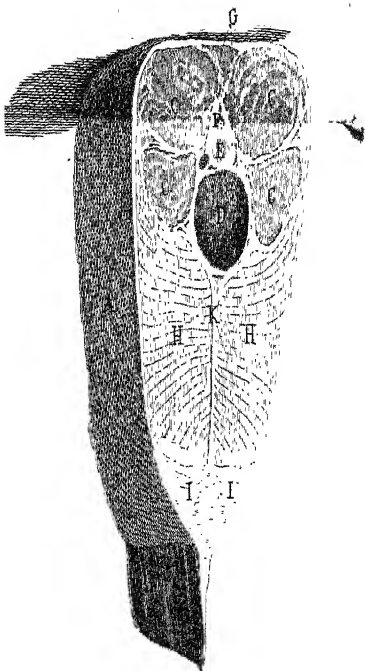
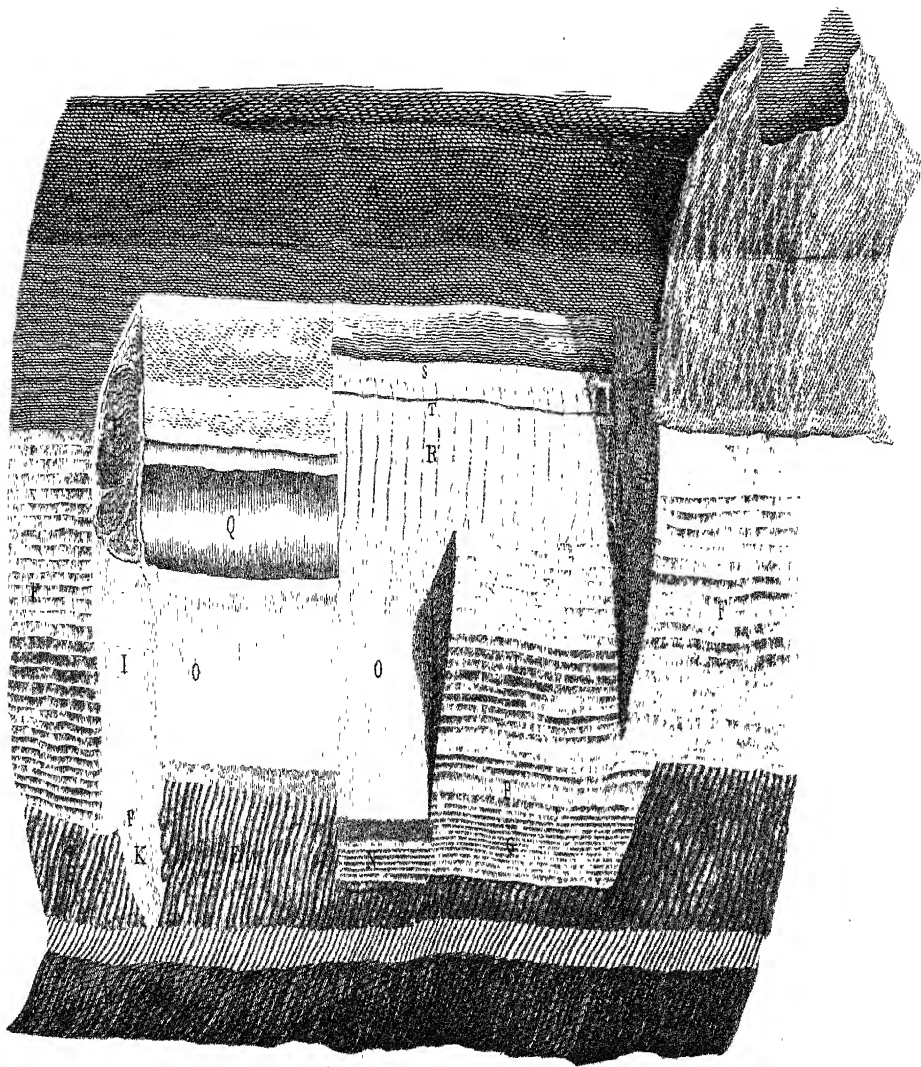


Fig 1





feated parts. II. The cut ends of the large organ, part of which has also been removed, to expose the deeper seated parts. K. The cut end of the small organ. L. A part of the large organ, the rest having been removed. M. The cut end of the above section. N. A section of the small organ. OO. The middle partition which divides the two large organs. P. A fatty membrane, which divides the large organ from the small. Q. The air-bladder. R. The nerves going to the organ. S. The *medulla spinalis*. T. The singular nerve.

P L A T E IV. F I G. V.

A transverse section of the fish, exposing at one view, all the parts of which it is composed. A. The external surface of the side of the fish. B. The under fin. CCCC. The cut ends of the muscles of the back. D. The cavity of the air-bladder. E. The body of the spine. F. The *medulla spinalis*. G. The large artery and vein. HH. The cut ends of the two large organs. II. The cut ends of the two small organs, K. The partition between the organs.



XL. *Some Observations upon Myrrh, made in Abyssinia, in the Year 1771, and sent to William Hunter, M. D. with Specimens, in February, 1775. By James Bruce, Esq.*

Redde, June 1, 1775. **T**HE ancients, and particularly PROSCRIDES, have spoken of myrrh in such a manner, as to leave us no alternative, but to suppose either that they have described a drug which they had never seen; or, that the drug seen and described by them is absolutely unknown to modern naturalists and physicians. The Arabs, however, who form the link of the chain between the Greek physicians and ours, in whose country the myrrh was produced, and whose language gave it its name, have left us undeniable evidence, that what we know by the name of myrrh, is in nothing different from the myrrh of the ancients, growing in the same countries from which it was brought formerly to Greece; that is, from the East coast of Arabia Felix, bordering on the Indian Ocean, and that low land in Abyssinia on the South-east of the Red-sea, included nearly between the 12th and 13th degree of North latitude, and limited on the West by a meridian passing through the island Massowa; and on the East by another, passing through Cape Guardfoy, without the straits of Bab el Mandel. This country the Greeks knew by the name  
of

of the Troglodytria; not to be confounded with another nation of Troglodytes, very different in all respects, living in the forests between Abyssinia and Nubia. The myrrh of the Troglodytes was always preferred to that of Arabia; and it has maintained this preference to our days. That part of Abyssinia being half over-run and settled, half wasted and abandoned, by a barbarous nation from the Southward, very little correspondence or commerce has been since carried on between the Arabians and that coast; unless by some desperate adventures of Mahometan merchants, made under favourable and accidental circumstances, which have sometimes succeeded, and very often likewise have miscarried. The most frequent way by which this Troglodyte myrrh is exported, is from Massowa, a small Abyssinian island, on the coast of the Red-sea. But notwithstanding this, the quantity of Abyssinian myrrh is so very small, in comparison of that of Arabia sent to Grand Cairo, that we may safely attribute to this only the reason, why our myrrh is not so good in quality as the myrrh of the ancients, which was Abyssinian. Although those barbarians make use of the gum, leaves, and bark, of this tree, in diseases to which they are subject; yet, as very little is wanted for such purposes, and the tree is the common timber of the country, this does not hinder them from cutting it down every day, to burn for the common uses of life; and as they never plant, or replace the trees destroyed, it is probable, that in some years the true Troglodyte myrrh will not exist; and the erroneous de-

scriptions of the Greek physicians will lead posterity, as they have done us now, into various conjectures, all of them false, upon the question, what that myrrh of the ancients was?

Though the myrrh of the Troglodytes was superior to any Arabian, yet the Greeks perceived, that it was not all of equal goodness. PLINY and THEOPHRASTUS make this difference to arise from the trees being partly wild, partly cultivated. But this is an imaginary reason; all the trees were wild. But it was the age of the tree and its health, the manner of making the cut or wound in it, the time of gathering the myrrh, and the circumstances of the climate when it was gathered, that constantly determined, and does yet determine, the quality of the drug. In order to have myrrh of the first, or most perfect sort, the savages chuse a young, vigorous tree, whose bark is without moss, or any parasite-plant; and, above the first large branches, give the tree a deep wound with an axe. The myrrh which flows, the first year, through this wound, is myrrh of the first growth; and never in very great quantity. This operation is performed some time after the rains have ceased; that is, from April to June; and the myrrh is produced in July and August. The sap once accustomed to issue through this gash, continues so to do spontaneously, at the return of every season: but the tropical rains, which are very violent, and continue six months, wash so much dirt, and lodge so much water in the cut, that in the second year, the tree has begun to rot and turn foul in that part, and the myrrh is of a second

cond quality, and sells in Cairo about a third cheaper than the first. The myrrh also produced from gashes near the roots, and in the trunks of old trees, is of the second growth and quality, and sometimes worse. This, however, is the *good* myrrh of the Italian shops every where but in Venice. It is of a blackish red, foul colour, solid and heavy, losing little of its weight by being long kept; and it is not easily distinguished from that of Arabia Felix. The third and worst kind is gathered from old wounds or gashes, formerly made, in old trees; or myrrh that, passing unnoticed, has hung upon the tree ungathered a whole year; black and earth-like in colour, and heavy, with little smell and bitterness. This apparently is the *caucalis* of the ancients.

PLINY speaks of *stacte*, as if it was fresh or liquid myrrh; and DIOSCORIDES, in his chapter upon it (cap. 67.), says something like this also. However, it is not credible, that the ancients, either Greeks or Latins, placed at such a distance, could ever see the myrrh in that state. The natives of its country say, that it hardens on the tree instantly, on being exposed to air; and I, who was several months within four days journey of the place where it grew, and had the savages quite at my devotion to go and come from thence, could never see the newest myrrh softer than the state it now is in; though, I think, it dissolved more perfectly in water, than when it had been kept. DIOSCORIDES too mentions a kind of myrrh which, he says, was green, and of the consistence of paste. But as SERAPION and the Arabs say, that *stacte* was

was a preparation of myrrh dissolved in water, it is probable, that this unknown green kind of DIOSCORIDES was, like the *stacte*, a composition of myrrh and some other ingredient, not a species of Abyssinian myrrh, which he could never have seen, either soft or green.

It may be remarked, that when we buy fresh or new myrrh, it has always a very strong, rancid, oily smell; and when thrown into water, *globules* of an oily matter swim upon the surface. This greasiness is not from the myrrh; it is owing to the savages using goats-skins anointed with butter (to make them supple) wherein to put their myrrh at gathering; and in these skins it remains, and is brought to market: so that, far from its being a fault, as some ignorant druggists at Rome and Venice believe, it is a mark that the myrrh is fresh gathered, which is the best quality that myrrh of the first sort can have. Besides, far from hurting the myrrh, this oily covering must rather at first have been of service; as it certainly imprisons and confines the volatile parts of new myrrh, which escape in great quantities, to a very considerable diminution in the weight. The piece of myrrh which I send you, is what a fine tree, less than fifteen inches diameter in the trunk at the bottom, wounded in two places, produced at one of the wounds, in the year 1771. And it may be regarded as the only unexceptionable and authentic evidence, in Europe, of what the Troglodyte myrrh was; unless it be those pieces still remaining in my collection, and a piece, somewhat smaller than yours, which I gave to the king of France's

France's cabinet at Paris. This piece which I send you, had lost near six dragmes Troy of its weight, between the 27th of August, 1771, and the 29th of June, 1773. It has lost a very few grains since. It was kept, as were all the other pieces, with great care in cotton, separately in a box, to prevent its losing weight by friction.

#### O P O C A L P A S U M.

At the time when I was on the borders of the Tal-Tal, or Troglodyte country, I sought to procure myself branches and bark of the myrrh tree, enough preserved to be able to draw it; but the length and ruggedness of the way, the heat of the weather, and the carelessness and want of resources of naked savages, always disappointed me. In those goat-skin bags into which I had often ordered them to put small branches, I always found the leaves mostly in powder; some few that were entire, seemed to resemble much the *acacia vera*, but were wider towards the extremity, and more pointed immediately at the end. In what order the leaves grew, I never could determine. The bark was absolutely like that of the *acacia vera*; and among the leaves I often met with a small straight weak thorn, about two inches long. These were all the circumstances I could combine, relative to the myrrh tree, too vague and uncertain to risk a drawing upon, when there still remained so many *desiderata* concerning it; and as the king was obstinate not to let me go thither, after what had happened to the sur-

geon, mate, and boat's crew, of the Elgin Indiaman, I was obliged to abandon the drawing of the myrrh tree to some more fortunate traveller. At the same time that I was taking these pains about the myrrh, I had desired the savages to bring me all the gums they could find, with the branches and bark of the trees that produced them. They brought me, at different times, some very fine pieces of incense, and at another time, a very small quantity of a bright colourless gum, sweeter on burning than incense; but no branches of either tree, though I found this latter afterwards, in another part of Abyssinia. But at all times they brought me quantities of gum, of an even and close grain, and of a dark-brown colour, which was produced by a tree called *sassa*: and twice I received branches of this tree in tolerable order; and of these I made a drawing. Some weeks after, walking in a Mahometan village, I saw a large tree, with the whole upper part of the trunk and the large branches so covered with great bosses and knobs of gum, as to appear monstrous: and asking farther about the tree, I found that it had been brought, many years before, from the myrrh country by merchants, and planted there for the sake of its gum, with which these Mahometans stiffened the blue Surat cloths, which they got damaged from Mocha, to trade in with the Galla and Abyssinians. Neither the tree which they called *sassa*, nor the name, nor the gum, could allow me to doubt a moment that it was the same as what had been brought to me from the myrrh country; but I had the additional satisfaction to find the

tree

tree all covered over with beautiful crimson flowers, of a very extraordinary and strange construction. I began then a drawing anew, with all that satisfaction known only to those who have been conversant in such discoveries. I took pieces of the gum with me. It is very light. GALEN complains, that in his time, the myrrh was often mixed with a drug which he calls *opocalpasum*, by a Greek name; but what this drug was, is totally unknown to us at this day. But, as the only view of the savage, in mixing another gum with his myrrh, must have been to increase the quantity, and as the great plenty, in which this gum is produced, and its colour make it very proper for this use; and above all, as there is no reason to think, there is another gum-bearing tree of equal qualities in the country where the myrrh grows, it seems to me next to a proof, that this must have been the *opocalpasum*. I must, however, confess, that GALEN says, the *opocalpasum* was so far from an innocent drug, that it was a mortal poison, and had produced very fatal effects. But as those Troglodytes, though now more ignorant than formerly, are still well acquainted with the properties of their herbs and trees, it is not possible, that the savage, desiring to increase his sales, would mix them with a poison that must needs diminish them. And we may therefore, without scruple, suppose, that GALEN was mistaken in the quality ascribed to this drug; and that he might have imagined, that people died of the *opocalpasum*, who perhaps really died of the physician. First, because we know of no gum or *resin* that is a

Vol. LXV.                      K k k                      mortal



mortal poison: secondly, because, from the construction of its parts, gum is very ill adapted for having the activity which violent poison has; and considering the small quantities in which myrrh is taken, and the *opocalpasum* could have been but in an inconsiderable proportion to the myrrh, to have killed, it must have been a very active poison. Thirdly, these accidents, from a known cause, must have brought myrrh into disuse, as certainly as the Spaniards mixing *arsenic* with the bark, would banish that drug when we saw people die of it. Now this never was the case: it maintained its character among the Greeks and the Arabs, and so down to our days; and a modern physician thinks it might make man immortal, if it could be rendered perfectly soluble in the human body. GALEN then was mistaken as to the poisonous quality of the *opocalpasum*. The Greek physicians knew little of the natural history of Arabia; less still of that of Abyssinia; and we who have followed them know nothing of either. This gum being put into water swells and turns white, and loses all its glue; it resembles gum *adragant* much in quality, and may be eaten safely. This specimen came from the Troglodyte country in the year 1771: a piece of myrrh from Arabia Felix, and a piece of gum of the *sassu* from Abyssinia were packed up in another separate box to be sent you for comparison, but forgotten by my servant. They will be sent hereafter. The *sassu*, the tree which produces the *opocalpasum*, does not grow in Arabia. Arabian myrrh is easily known from Abyssinian by the following method: take

take a handful of the smallest pieces, found at the bottom of the basket where the myrrh was packed, and throw them into a plate, and just cover them with water a little warm; the myrrh will remain for some time without visible alteration, for it dissolves slowly; but the gum will swell to five times its original size, and appear so many white spots amidst the myrrh.

The pieces sent you are, N° 1. Virgin Troglodyte myrrh. N° 2. The worst sort of Troglodyte myrrh, called *cancabs*. N° 3. *Opocalpasum* from the myrrh-country.

*XLII. An Account of a curious Giant's Causeway, or Group  
of angular Columns, newly discovered in the Euganean  
Hills, near Padua, in Italy. In a Letter from John  
de Strange, Esq. F. R. S. to Sir John Pringle, Bart. P. R. S.  
See your account of the Giant's Causeway, &c. in the  
to exhibit a new and singular phenomenon.*

TO SIR JOHN PRINGLE, BART. P. R. S.

SIR,

Venice, March 10, 1775.

Redde, June 15,

**A**S you thought proper, to recommend  
to the notice of our learned Society  
the account, which I did myself the honour of commu-  
nicating to you not long since, of two giants causeways  
in this Venetian state; I now take the liberty to send you  
the description and figure of another similar phænome-  
non, equally curious, and lately discovered in the same  
neighbourhood. It is situated at Castel Nuovo, a small vil-  
lage near Teolo, also in the Euganean hills, about four  
miles South-west of the other Giant's Causeway of Monte  
Roffo before described. I am indebted for the intelli-  
gence of this new causeway to the ingenious Abbé FOR-  
TIS, whom curiosity also led among those hills; and who,  
at my request, accompanied a painter I lately sent from  
hence, to make the drawing of it, which I have now the  
pleasure to transmit to you<sup>(a)</sup>. *Il Sasso di San Biasio,*

(a) Plate XI.

c

which

which is the name of the spot where this causeway is situated, is a large insulated rock, composed of the same sort of grey granite that is common to the Euganean hills, and which I have before described<sup>(b)</sup>. The columns which form this causeway, partly against the flank of the rock, and partly round its base, are of the same substance, with the rock itself, to which they adhere, as I have constantly observed in all similar groups. They are therefore of a compound nature, like the columns of Monte Rosso, and differ intirely from the common sort, which are mostly homogeneous, or of an uniform texture; as is observable in the jointed, as well as simple species of *basalttes*. I shall take the first opportunity of sending a fragment of one of the newly discovered columns, for the inspection of the Society; in the mean time the inclosed pieces, which were broken from one of them, will serve to shew, how different their substance is from that of the common basaltic columns. By comparing these pieces with the fragments of the columns of Monte Rosso which I before transmitted to the Society, some essential difference will appear between them. Those of San Biazio, though very hard, are rather porous, of a lighter colour than the columns of Monte Rosso, and very much resemble a species of *lava*, which I have often seen. This porousness I also remember to have once before observed, and more signally too, in some basaltic columns near Achon, in the province of Auvergne, in France. The pores in the columns of both

(b) See Article II. of this volume.

these groups are also irregularly dispersed, and of unequal size, like those of pumice stones and other common *pori ignei*. Those of the columns of San Bialio are moreover commonly invested with a sort of *crocus martis*, which I have also frequently observed in the pores of other volcanic concretions. These properties are surely further marks in favour of the igneous origin of such columnar crystallizations; especially, since they seem contrary to the principle by which the common aqueous crystals are formed, successively, *et per juxtapositionem partium ad partes*. In fact, these crystals manifest no such porosity. I also observed, that the columns of Achon, though of a homogeneous substance, yet differ from the common *basaltes* by their immense size as well as colour, which is rather brown than black. The columns of San Bialio are likewise very large, measuring often two feet in diameter. They are also of the simple species, that is not jointed, and mostly quadrangular, which figure seems rather a principal characteristic of this group, being rarely observed in others. So true it is, as I formerly remarked, that some particular characteristic ever distinguishes the different groups of *basaltes*; which, therefore, cannot be too narrowly observed, before we pretend to form any opinion about their origin. Some few, but very few, chiefly of the smaller columns of San Bialio, are of a pentagonal form, like the specimen which I propose to transmit to the Society. But there are no hexagonal columns, which, in other

other balaltic groups, are the most common. The natural position of these columns, whether facing the rock, or about the bottom of it, is mostly perpendicular. Another adjacent portion of this rock is also characterized by angular and, as it were, winding *strata*, somewhat resembling the bending pillars of Staffa, as may be observed in the drawing. The rock itself is also composed of angular masses, as are indeed most granites; and these masses are also ranged perpendicularly. Several emerge, as it were, from the tops and sides of the neighbouring rocks and hills, like so many stately and artificial pillars. The winding *strata* before mentioned are also parallel with each other, as I have frequently observed in other granites, as well as common volcanic *strata* in general, particularly of the harder sort. DESMAREST calls the latter *Basaltes en tables*(<sup>c</sup>); which is a kind of volcanic slate, formed in parallel *strata* of different thickness, from two or three to five and six inches. This is very common in the provinces of Velay and Auvergne, in France, where it is also used for coverings of houses. The same sort of slate is likewise common to the mountains of Genoa, many of which seem to be of volcanic origin, as I recollect to have remarked in passing the chain of the *bochetta*, between Genoa and the plain of Lombardy. I mention this circumstance, as the volcanic *phenomena* of that part of Italy have not hitherto been attended to. In fact, it is lately only that such observations begin to be made in other countries; the characters of extinct volcanos or volcanic

(c) Encyclopedie, Art. Pavé des Giants.

tracts, being but little known, though such tracts seem to occupy every where a very considerable part of the surface of the earth. I remember to have observed these flaty tables, or parallel *strata*, of granite, near the top of the famous San Gothard, in the ascent of that mountain on the side towards Switzerland. These *strata* are also ranged perpendicularly, like the other common ones in granites, and resemble DESMAREST's *basaltes en tables*; affording thus another proof of the analogy remarkable between the organization of the different masses in granites, and that of common volcanic *strata* in general. The former, as well as the latter, have their prismatic columns, their *basaltes en tables*, as DESMAREST calls them, and *en boules*, as I have observed in my account of Monte Rosso. Surely, therefore, these are strong proofs in favour of the common origin of both. The rocks of San Biaſio abound with ferruginous vitrifications, which are frequently observable in granites; and the neighbouring tracts with *lava* or *pori ignei*; as I have also observed, when I made the tour of this country, particularly about Teolo. The Abbé FORTIS brought me a piece of *lapis lenticularis*, broken from the limestone that superficially covers the granite of these Euganean hills, in many places, as I before observed. I mention this circumstance, recollecting to have taken notice, in my last paper, that such figured bodies are not commonly found in the limestone of this country. As the present account may serve, by way of appendix, to that which I lately did myself the honour to present











present to the Society, through your favour; you will oblige me also by the communication of it to that learned body, should you find it deserving of their attention.

I have nothing further to add at present, but the assurances of my being, with great truth and esteem, SIR,

Your most obedient humble servant,

JOHN STRANGE.

XLII. *Observations on the Difference between the Duration of Human Life in Towns and in Country Parishes and Villages.* By the Rev. Richard Price, D. D. F. R. S.  
Communicated by Dr. Horsley.

Redde, June, 22, 1775. **T**HIS Society has lately been much obliged to Dr. PERCIVAL, for the accounts he has communicated of the state of population at Manchester and its adjacent places. These accounts contain some facts, which appear to me curious and important. From the last in particular, there appears to be reason for concluding, that whereas a 28th part of the inhabitants die annually in the town of Manchester, not more than a 56th part die annually in the adjacent country. This implies a difference so great between the rates of human mortality in these different situations, that some, whose judgements I reverence, have thought it incredible. I will, therefore, beg leave to offer the following observations on this subject.

In the first place, the evidence in this instance is such as seems to leave little room for doubt. From an accurate survey it appears, that the number of inhabitants in the town was 27246, in the year 1773. The number of deaths the same year (and also the average for 1772, 1773,

1773, and 1774), was 973<sup>(a)</sup>; that is, a 28th part of the number of inhabitants. From an equally careful survey it appears, that the number of inhabitants in that part of the parish of Manchester which lies in the country, was 13786. The number of deaths in 1772 was 246; that is, a 56th part of the number of inhabitants. The chief objection to this evidence is, that the number of deaths in that part of the parish which lies in the country is given only for one year; whereas the average of several years ought to be given. But first, the number of deaths in 1772, in the town, was nearly the same with the medium for seven years; and from hence there arises a probability, that in the adjacent country, the number of deaths, in the same year, could not have been much lower than the medium. Secondly, supposing it lower, there is the highest probability, that it was not more than a 4th or 5th lower. Suppose then the true annual medium to be 300, instead of 246, and it will follow, that whereas a 28th part of the inhabitants die in the town annually, a 46th part die in the country; and this is a difference very considerable. But farther, I would

(a) The numbers of burials in the town, including the addition of 50 every year for dissenters, was in 1772,

1773,

1774,

954

973

1008

Within the parish, but out of the town, there are 13 episcopal and dissenting chapels; and the number of burials in all these chapels, in 1772, was 246. The christenings were 401. The number of burials brought from the country into the town is not considerable; and it is, I am informed, pretty exactly balanced by the burials carried out of the town into the country.

observe, that the difference which this survey gives between the rate of mortality in the town of Manchester and the adjacent country, is confirmed by a variety of other accounts. It may be stated in general, that whereas in great towns, the proportion of inhabitants dying annually is from 1 in 19 to 1 in 22 or 23, and in moderate towns from 1 in 24 to 1 in 28 <sup>(b)</sup>; in country parishes and villages on the contrary, this proportion seldom exceeds 1 in 40 or 50. The proofs of this are numerous and unexceptionable; and I have elsewhere given a particular account of them <sup>(c)</sup>. I will here only mention the following facts.

The number of inhabitants at STOCKHOLM in 1763 was 72979. The average of deaths for the six preceding years had been 3802 <sup>(d)</sup>. One, therefore, in nineteen died there annually.

At

(b) The number dying annually in towns is seldom so low as 1 in 28, except in consequence of a rapid increase produced by an influx of people, at those periods of life when the fewest die. This is the case at Manchester. It is also the case at Liverpool and at Berlin; in the former of which towns, 1 in 27 dies annually; and in the latter, 1 in 26½ died from 1755 to 1759. See *Observations on Reverfionary Payments*, p. 224, &c. 3d edition.

(c) See *Observations on Reverfionary Payments*, &c. Essay 1st, and Supplement.

(d) See a Memoir by M. WARGENTIN, in the 15th volume of the *Collection Academique*, printed at Paris, 1772. From this memoir I learn, that in 1757, and 1760, and 1763, a survey was made of the inhabitants of Sweden, distinguishing, particularly, the numbers of both sexes living at every age; and that also, for nine years (or from 1755 to 1763), an exact register was kept of the number of births and burials in each year, distinguishing the age and sex of every one that died. I do not know whether this regulation has been continued

to

At ROME, an account is taken every year of the number of inhabitants; and, in the year 1771, it was 159675. The average of deaths for ten years had been 7367: one, therefore, in 21½ died annually.

In LONDON I have shewn, with an evidence which I think little short of demonstration, that at least 1 in 20¾ of the inhabitants die annually<sup>(c)</sup>. And, from a particular survey and a very accurate register of mortality at NORTHAMPTON, it appears, that 1 in 26½ die there annually.

Let these facts be compared with the following. In 1767, a survey was made of the inhabitants of the island of MADEIRA, under the direction of Dr. THOMAS HEBERDEN, and their number was found to be 64614. The average of burials for eight preceding years had been 1293. Only 1 in 50, therefore, of the inhabitants died annually (see Philosophical Transactions, vol. LVII. p. 461.).

to the present time; but the result of it, for the nine years I have mentioned, as given by M. WARGENTIN in this Memoir, contains indeed a most curious account of the state of population in Sweden; and it is particularly to my present purpose to mention, that it shews, that though a 19th part of the inhabitants of Stockholm die every year, yet in the whole kingdom, taking all the towns and country together, not more than a 35th part die every year. In 1757, Sweden consisted of 1101595 males and 1221600 females; in 1760, of 1121053 males and 1246445 females; and in 1763, of 1165489 males and 1280925 females. The annual average of births, from 1755 to 1763, was 46223 males and 44017 females; of marriages, 21219; of deaths, 34088 males and 35037 females.

(c) See Observations on Reversionary Payments, Essay IV. p. 253, &c.

The



The district of VAUD, in Switzerland, in 1766, contained 112951 inhabitants. The average of deaths for ten preceding years had been 2504. Only 1 in 45, therefore, died annually *(f)*.

The number of inhabitants in the parish of ACKWORTH, in the county of York, in 1757, was 603; and the average of deaths for ten years had been  $10\frac{7}{10}$ , or a 56th part. In 1767, the inhabitants were increased to 728; and the annual average of deaths was  $15\frac{3}{10}$ , or nearly a 47th part *(g)*.

The reason of this striking difference between the rate of human mortality in towns and in country parishes and villages must be, first, the luxury and the irregular modes of life which prevail in towns; and, secondly, the foulness of the air. But it has been inquired, whether the migrations of people from the country to towns may not produce this difference, by lessening the proportion of inhabitants that die in the country, and increasing the same proportion in towns? In answer to this enquiry I would observe: first, that this difference being a difference of near a half, it is apparently much greater than can be accounted for by any such cause. But, secondly, it should be confi-

*(f)* See M. MURET's Memoir on the State of Population, in the *Pays de Vaud*, printed at Bern, in 1766; and the Supplement to the Observations on Reversionary Payments, p. 358. 3d edit.

*(g)* I owe this information concerning the parish of Ackworth to a curious register kept there by Dr. LEE. I have taken the liberty to insert this register in the postscript, together with the annual register and survey of Rome from 1762 to 1771.

dered, that if migrations lessen the number of deaths, they also lessen the number of inhabitants; and that it depends intirely on the ages at which the inhabitants remove from a place, whether the effect of their removal shall be lowering or raising the proportion of the annual deaths to the number of inhabitants. In the present case, the truth appears to be, that the most common age of migration from the country is such as raises this proportion in the country. This will be evident from the following considerations. The period of life in which persons remove from the country to settle in towns, is chiefly the beginning of mature life, or from the age of 10 or 15 to 25 or 30. In infancy, none migrate; and in the decline of life, it is more usual to retire from towns than to remove to them. Towns, therefore, will be inhabited more by people in the firmest parts of life; and, on the other hand, the country will be inhabited more by people in the weakest parts of life; and the consequence of this is, that in the country, the inhabitants must die faster in proportion to their number than they otherwise would, and that in towns they must die more slowly. In particular, the number of children is always much greater in the country than in towns; and this is a circumstance which must be extremely unfavourable to the former: for it is well known, that there are no years of life, in which so many of a given number die, as the first three or four years. Till the age of five, human life, like a fire beginning to burn, is very feeble; and in some situations more than half, and in others, a third or fourth of all that are born die before that age. After this, life grows

grows less and less precarious till it acquires its utmost vigour at 10 or 15; and of the living at this age, not above 1 in 70 or 80 dies annually in the worst situations; and in the best situations, not above 1 in 150 or 160. After 15, life declines, and continues to do so more and more, till it becomes quite extinct in old age. If, therefore, in any situation, the inhabitants consist more of persons in mature life, and yet die faster, it must be owing to some particular causes of mortality that operate there. This is the case in all towns where any observations have been made. Manchester, in particular, is not only kept up, but increases fast, by removals to it of persons in the prime of life. The country round it increases likewise; but it is by an excess of the births above the deaths; that is, by accessions to it of children in the very feeblest part of life. This ought to raise the proportion of annual deaths to inhabitants in the country, much above the same proportion in the town; but, instead of this, it is near one-half lower.

It may be needless to add any thing to these observations.

In order, however, to put this matter out of all doubt, I will observe farther, that it appears in fact, from the accounts furnished by Dr. PERCIVAL, that the number of inhabitants in the periods of life when mankind die fastest<sup>(n)</sup> (that is, in the first and last stages of life), is con-

(n) In towns, about a fourth of the inhabitants die commonly between 14 and 51; a fifth or sixth die at 51 and upwards; and the remainder die under 15. In country parishes and villages about a fifth die between 14 and 51; about two-fifths at 51 and upwards; and the remainder under 15.

siderably less in the town of Manchester than in the adjacent country. The number of inhabitants in the town, under 15 and above 50, is 13467; in the country, 7305. And the whole number is, in the town, 27246; in the country, 13786. In the town, therefore, the inhabitants, in the first and last stages of life, do not make half the whole number; but in the country, they make considerably more than half. At Ackworth, likewise, in Yorkshire, the inhabitants under 15 and above 50 are more than half the whole number; and the same is true at Hale near Altringham, at Horwich, at Darwen near Blackburn in Lancashire, and at Cockey Moor<sup>(i)</sup> near Bolton,

(i) I am much indebted to Dr. PERCIVAL for the following account of these places. The society belonging to the chapel at Hale is composed of 140 males, 136 females, 92 married persons, 8 widowers, 12 widows, 105 under 15, and 41 above 50. The deaths, during seven years, have been 28, and the births 68. Mr. EVANS's congregation at Horwich, consists of 305 individuals; *viz.* 149 males, 156 females, 94 married persons, 9 widowers, 8 widows, 127 under 15 years of age, and 50 above 50. The births, for seven years, 101; the deaths 32. A 66th part, therefore, die annually in both these places. The rev. Mr. SMALLEY's congregation at Darwen, consists of 1850 individuals; *viz.* 900 males, 950 females, 640 married persons, 30 widowers, 48 widows, 737 persons under the age of 15, and 218 above 50. During the last seven years the births have amounted to 508, the deaths to 233. A 56th part, therefore, die annually. Mr. BARNES's congregation at Cockey Moor, consists of 154 families and 711 individuals; namely, 320 males, 391 females, 248 married persons, 10 widowers, 27 widows, 252 persons under the age of 15, and 99 above 50. Deaths in seven years 114; in which period the deaths were considerably increased by an uncommon fatality of the small-pox. One person in 44 died annually. The rev. Mr. MERCER's congregation at Chowbent, in Lancashire, consists of 1160 persons; *viz.* 554 males, 606 females, 173 males and 150 females under the age of ten, 83

Bolton, in the same county; and yet in some of these places it appears, that not a 60th part of the inhabitants die annually.

At Stockholm, in 1763, the inhabitants under the age of 5, were only a 12th; above 70, only a 46th part of the whole number. But in all Sweden, the number under 5 was a 7th; and above 70, near the 32d part of all the inhabitants: and yet 35 die in the town to 19 in the whole kingdom. This may be easily deduced from Mr. WARGENTIN's tables in the *Collection Academique* before quoted.

To the accounts which give the proportion of inhabitants to annual deaths so high as 50 or 60 to 1, it has been farther objected, that if true, it must follow, that in such situations half the inhabitants must live to 50 or 60 years of age. But were this a right inference, there would be nothing in it incredible. For though in most cities one-half die in the first two or three years after birth; yet, in many country situations, the greater part live to marry: and in the parish of Ackworth, particularly, it appears with undeniable evidence from the register, that one-half of all born there live to the age of 46. It appears also, with equal evidence, from M. MURET's tables in the Bern Memoirs for 1766, that in 43 parishes in the district of Vaud, one-half of all born there

males and 9 females above 50, 398 married persons, 26 widowers, and 43 widows. The baptisms during six years, wanting six weeks, have amounted to 293, and the deaths to 169. One person, therefore, in 41 died annually. These surveys were made in the year 1773.

live

live beyond the age of 41. In truth, did all mankind lead natural and virtuous lives, that waste of the species which happens in infancy and childhood would not take place, and few would die except in old age. The inference, however, which I have mentioned, cannot be made with reason. It is just only in the particular case of an uniform decrease in the probabilities of living from birth to old age; and this is a case that has never existed. In all other cases, there is not any necessary connexion between the proportion of inhabitants dying annually, and the age to which the greater part live. In most cities one-half, as I have just observed, of all that are born die before two or three years of age. But it cannot be imagined, that there is any place where so many as one-half or a third of the inhabitants die every year.

But to return to Dr. PERCIVAL's account of the town and parish of Manchester. It appears from this account, that the number of children under 15, compared with the number of inhabitants between 14 and 51, is greater in the country than in the town of Manchester, in the proportion of no less than 5 to 4<sup>(k)</sup>. It follows, therefore, that though, in consequence of a constant influx of people to the town, it is more filled than the country with

(k) In the town, the number of inhabitants between 14 and 51 is 13779; and 9575 under 15. In the country, the former number is 6481; and the latter, 5545. But the last number would have been only 4503, had the proportion of the inhabitants between 14 and 51 to the inhabitants under 15 been the same in both situations. It is owing to this, that the number of persons in a family in the country is  $5\frac{1}{2}$ ; but in the town only 4 $\frac{1}{2}$ .

inhabitants in the most vigorous periods of life; yet one child in four less is born in the town than in the country. This is a remarkable circumstance, and the reasons of it must be the two following. First, the town inhabitants being less healthy, and dying faster, have not the same strength of constitution with the country inhabitants. Secondly, in the town a smaller proportion of the adult inhabitants marry; and they marry later than in the country. The survey fully proves this; for it appears, that though the number of inhabitants at the most common marrying ages, compared with the whole number of the living above the age of 14, is smaller in the country than the town; yet the proportion of the married to the living above 14, is very nearly the same in both situations. And there are more widows and widowers in the town than in the country in the proportion of near 16 to 11. We learn from hence, I think, clearly in what manner towns operate in checking population, and preventing the increase of mankind.

Dr. PERCIVAL informs us, that the reverend and learned Dr. TUCKER has been led, by some observations he has made at Bristol, to doubt whether the common opinion is right, with respect to the disproportion between the number of male and female births; and that he, therefore, wishes a farther inquiry may be made into this subject. This has induced me to collect the following facts, which, I think, will abundantly settle this point.

	Born Males.	Females.	Proportion.
In London for the last 110 years, or from 1664 to 1773,	862293	817072	20 to 19
Paris, for 8 years (1),	79693	76481	25 to 24
Leyden, for 50 years (m),	46773	44933	26 to 25
Vienna, for 27 years, ending 1746 (n),	67060	64893	31 to 30
Berlin, for 40 years, ending 1761 (o),	71188	67431	20 to 19
Kurmark of Brandenburg, for 9 years, ending 1759 (p),	102425	96521	18 to 17
Dukedom of Magdeburgh, for 38 years, ending 1759 (q),	153227	145985	21 to 20
All the Prussian towns, for a course of years, (r),	691826	659072	21 to 20
In a great number of country parishes, for a course of years (s),	59067	56282	21 to 20
In the same country parishes, for another period of years (t),	89530	84954	19 to 18
Leeds, Manchester, Coventry, &c. for a period of years (u),	108784	103449	20 to 19
In the same towns, for another period (x),	57084	54128	20 to 19
Total,	2388950	2271201	20 to 19
Sweden, for 9 years, ending 1763,	416007	396124	20 to 19

Mr. DERHAM, in his *Physico-Theology*, p. 175, has stated the proportion of male to female births at 14 to 13, and this proportion has ever since been generally received as the true one; but it appears from this table, that it ought to have been stated at 20 to 19. But though it appears, that the number of males born is in this proportion greater than the number of females born, yet, in most places, the number of males living has been

- (1) See *SUSM. GORTLICK'S Ordinance Tables*, p. 16. (m) *Ibid.* p. 17.  
 (n) *Ibid.* p. 13. (o) *Ibid.* p. 12. (p) *Ibid.* p. 3. (q) *Ibid.* p. 5.  
 (r) *Ibid.* p. 9. (s) See Dr. SHORT'S *New Observations*, p. 27. 31.  
 (t) *Ibid.* p. 30. (u) *Ibid.* p. 49. (x) *Ibid.* p. .

found



found to be less than the number of females. The reason is, without doubt, that males are more short-lived than females; and this is owing partly to the peculiar hazards to which males are subject, and their more irregular modes of life; but it is owing principally to some particular delicacy in the male constitution, which renders it less durable: for there are many observations which prove, that the greater mortality of males takes place chiefly in the first and last stages of life. A few facts of this kind I will beg leave to mention, because I have just met with them.

In the parish of St. Sulpice, at Paris, during 30 years, 5 males under a year old died to 4 females. But under 10, only 13 males died to 12 females (see SUSMILCH. Tables, vol. II. p. 30.)

In Stockholm, during 9 years ending in 1763, the number of still-borns amounted to 666; of whom 390 were males, and 276 females; that is, 10 to 7. The number of the living in the town above the age of 80 was, in 1760, 332; of whom 248 were females, and 84 males, or near 3 to 1. In the whole kingdom of Sweden, including all town and country inhabitants, the number of still-borns, during the 9 years just mentioned, was 19845; of whom 11424 were males, and 8421 females, or near 4 to 3. The number of the living in the whole kingdom consisted of more females than males, in the proportion of 10 to 9. It consisted of more females turned of 80 than males, in the proportion of 33 to 19; and of more females turned of 90 than males in the proportion

portion of near 2 to 1. (See M. WARGENTIN's Memoir in the *Collection Academique*, vol. XV.) Having now had occasion to refer again to this Memoir, I will just add, that it appears, that by the excess of the births above the deaths, Sweden gains every year an addition of above 20000 inhabitants; and that in six years they increased from 2323195 to 2446394. I am afraid, were regulations established for a similar inquiry in this kingdom, we should be far from finding our state so encouraging. London alone is a gulph which swallows up an increase equal to near three-fourths of that of Sweden.

### P O S T S C R I P T.

The following tables have been selected from several more of the same kind in M. WARGENTIN's Memoir on the state of population in Sweden. I have inserted them here, because they fully verify most of the observations in the preceding paper, and contain more distinct and authentic information on the subject of human mortality than I have ever before met with.

TABLE I.

Shewing the order of human mortality in SWEDEN.

	Annual deaths, being the average of three years, 1761, 1762, and 1763.		Number of the living in 1763.	
	Males.	Females.		
Still-born,	1324	988	Born,	47216 44892
Died under 1	11172	9850	Living under 1	36094 35453
Died between 1 and 3	4393	4336	Living betw. 1 and 3	66059 67234
3—5	2206	2249	3—5	66454 67711
5—10	2151	2057	5—10	130019 130758
10—15	933	834	10—15	126696 128021
15—20	711	658	15—20	108312 109985
20—25	834	756	20—25	92299 105115
25—30	883	863	25—30	88056 101003
30—35	1020	1146	30—35	85936 95811
35—40	955	923	35—40	74826 81413
40—45	1180	1170	40—45	67448 74854
45—50	1099	938	45—50	52398 59551
50—55	1280	1113	50—55	47298 56646
55—60	1177	1097	55—60	37086 45537
60—65	1586	1721	60—65	34892 44925
65—70	1237	1566	65—70	20649 28964
70—75	1322	2041	70—75	15454 23159
75—80	1092	1695	75—80	8858 13556
80—85	917	1446	80—85	4620 7487
85—90	414	650	85—90	1508 2694
Above 90	215	379	Above 90	527 988
Total of annual deaths,	36777	37488	Total of the living at all ages,	1165489 1280905

In this table it is observable, that the number of the living, in every equal division of life from birth, decreases continually till all become extinct; and that though the males born are more than the females born, in the proportion of 20 to 19; yet the males living of all ages are less in number, in the proportion of 1165489 to 1280905, or nearly of 10 to 11; notwithstanding which, the males that die annually are to the females as 52 to 53.

TABLE II.

Shewing the order of human mortality at STOCKHOLM.

		Annual deaths, being the average of three years, 1761, 1762, and 1763.		Number of the living in 1763.	
		Males.	Females.		
				Males.	Females.
Still-born,		54	43	Born,	1406 1340
Died under 1		567	489	Living under 1	684 733
Died between 1 and 3		161	170	Living betw. 1 and 3	1173 1348
	3—5	80	79		3—5 1022 1106
	5—10	71	72		5—10 2630 2774
	10—15	49	24		10—15 3151 2918
	15—20	53	30		15—20 3018 2865
	20—25	91	64		20—25 3070 4056
	25—30	121	78		25—30 3380 4251
	30—35	141	102		30—35 3705 4234
	35—40	118	96		35—40 3019 3288
	40—45	140	115		40—45 2846 3130
	45—50	101	84		45—50 1775 1984
	50—55	105	91		50—55 1581 2129
	55—60	61	54		55—60 853 1329
	60—65	79	88		60—65 826 1383
	65—70	41	54		65—70 370 778
	70—75	33	77		70—75 260 574
	75—80	28	59		75—80 128 324
	80—85	18	45		80—85 58 127
	85—90	7	20		85—90 16 51
Above 90		3	11	Above 90	10 22
Total of annual deaths,		2068	1902	Total of the living at all ages,	33575 39404

In this table it may be observed, that the number living at every age from birth decreases only till five. Between 5 and 10 Stockholm begins to receive recruits from the country, and they come in faster and faster till 35; after which age it appears, that more die than come in; and that the living in every subsequent period goes on decreasing continually till the end of life. It is farther observable, that this table exhibits a greater difference than the former, between the mortality of males and females.

A comparison of these tables will shew a striking contrast in other respects between the state of human mortality in the whole kingdom of Sweden and in its capital. In order to make this more obvious and unexceptionable, I will add the following table, deduced from all M. WARGENTIN's tables taken together.

T A B L E III.

In all SWEDEN for nine years.			In STOCKHOLM for 9 years.		
	Males.	Females.	Males.	Females.	
Still-born,	1 in 36	1 in 47	1 in 32	1 in 43 $\frac{1}{2}$	
Died under 1 of all born,	1 in 4 $\frac{1}{2}$	1 in 4 $\frac{1}{2}$	1 in 2 $\frac{1}{2}$	1 in 2 $\frac{1}{5}$	
Died annually of the } living betw. 1 and 3 }	1 in 17 $\frac{1}{2}$	1 in 17 $\frac{1}{2}$	1 in 7	1 in 7 $\frac{1}{2}$	
Between 3—5	1 in 34 $\frac{1}{2}$	1 in 36	1 in 13 $\frac{1}{2}$	1 in 16	
5—10	1 in 71	1 in 76	1 in 34 $\frac{1}{2}$	1 in 39	
10—15	1 in 149	1 in 161	1 in 79	1 in 114	
15—20	1 in 149	1 in 164	1 in 59	1 in 99	
20—25	1 in 108	1 in 139	1 in 44	1 in 79	
25—30	1 in 98	1 in 113	1 in 33	1 in 58	
30—35	1 in 85	1 in 84	1 in 31	1 in 43	
35—40	1 in 78	1 in 91	1 in 26 $\frac{1}{2}$	1 in 39	
40—45	1 in 56	1 in 63	1 in 23	1 in 31	
45—50	1 in 49	1 in 65	1 in 19 $\frac{1}{2}$	1 in 28	
50—55	1 in 37	1 in 50	1 in 16 $\frac{1}{2}$	1 in 25 $\frac{1}{2}$	
55—60	1 in 31	1 in 40	1 in 14	1 in 24	
60—65	1 in 23	1 in 26	1 in 11	1 in 16	
65—70	1 in 17	1 in 18 $\frac{1}{2}$	1 in 9 $\frac{1}{2}$	1 in 13 $\frac{1}{2}$	
70—75	1 in 11 $\frac{1}{2}$	1 in 11 $\frac{1}{2}$	1 in 7 $\frac{1}{5}$	1 in 8	
75—80	1 in 8	1 in 8 $\frac{1}{2}$	1 in 4 $\frac{1}{2}$	1 in 5	
80—85	1 in 5 $\frac{1}{2}$	1 in 5 $\frac{1}{2}$	1 in 3 $\frac{1}{2}$	1 in 3 $\frac{1}{2}$	
85—90	1 in 3 $\frac{1}{2}$	1 in 4	1 in 2	1 in 2 $\frac{1}{2}$	
Above 90	1 in 2 $\frac{1}{2}$	1 in 2 $\frac{1}{2}$	1 in 2 $\frac{1}{3}$	1 in 2 $\frac{1}{5}$	
Died of all living at all ages,	1 in 33 $\frac{1}{2}$	1 in 36	1 in 17 $\frac{1}{5}$	1 in 20 $\frac{1}{5}$	

A general Bill of all the CHRISTENINGS and BURIALS in the Parish of ACKWORTH, in the county of YORK, extracted from the Parish Register, for ten Years, from March 25, 1747, to March 25, 1757.

In ten Years Christened, Males 62. Females 65. Total, 127.				In ten Years Buried, Males 58. Females 49. Total, 107.			
	Males.	Females	Total.		Males.	Females.	Total.
Whereof have died				And there have died of			
Under two years old,	6	11	17	Apoplexy,	0	1	1
Between 2 and 5	1	2	3	Cancer,	1	0	1
5—10	2	2	4	Cholic,	1	0	1
10—20	1	2	3	Consumptions,	10	13	23
20—30	6	2	8	Dropfy,	4	1	5
30—40	2	3	5	Fevers,	23	12	35
40—50	11	3	14	Infants,	6	7	13
50—60	9	2	11	Lunacy,	0	1	1
60—70	9	7	16	Old Age,	9	15	24
70—80	9	8	17	Palsey,	1	0	1
80—90	1	6	7	Quinsy,	0	1	1
90—100	1	1	2	Small-Pox,	1	0	1
Of all, in 10 Years,	58	49	107	Of the above Dis- tempers, in 10 Y.	56	51	107

In this Parish there are { 160 Houses, twelve of which are uninhabited.  
603 Souls of the following Ages; viz.

	Males.	Females.	Total.		Males.	Females.	Total.
Under two years old,	12	19	31	Between 40 and 50	40	22	62
Between 2 and 5	25	19	44	50—60	38	33	71
5—10	30	38	68	60—70	25	14	39
10—20	59	58	117	70—80	4	8	12
20—30	55	41	96	80—90	4	0	4
30—40	26	33	59	90—100	0	0	0
Total of all Ages,					318	285	603



A general Bill of all the CHRISTENINGS and BURIALS in the Parish of ACKWORTH, in the County of YORK, for ten years, from March 25, 1757, to March 25, 1767.

In ten Years Christened, Males, 104. Females, 108. Total, 212.				In ten Years Buried, Males, 79. Females, 77. Total, 156.			
	Males.	Females.	Total.		Males.	Females.	Total.
Whereof have died				And there have died of			
Under two years old.	18	13	31	Apoplexy,	2	1	3
Between 2 and 5	9	7	16	Asthma,	2	1	3
5—10	4	1	5	Cancer,	0	1	1
10—20	2	2	4	Catastrophes,	5	1	6
20—30	7	5	12	Childbed,	0	2	2
30—40	3	8	11	Chincough,	0	2	2
40—50	2	4	6	Consumptions,	23	15	38
50—60	11	3	14	Convulsions,	4	2	6
60—70	13	13	26	Diabetes,	1	0	1
70—80	7	14	21	Dropty,	0	3	3
80—90	3	6	9	Dysentery,	1	1	2
90—100	0	1	1	Fever,	12	11	23
Of all Ages, in 10 Y.	79	77	156	Jaundice,	1	0	1
				Infants,	7	6	13
				Lunacy,	0	1	1
				Measles,	0	2	2
				Mortification,	2	1	3
				Old Age,	11	19	30
				Palfey,	1	0	1
				Quinsey,	1	0	1
				Small-Pox,	7	6	13
				Teeth,	0	1	1
				Of all the above Disorders, in 10 Years,	80	76	156

In this Parish there are { 184 Houses, eleven of which are uninhabited.  
728 Souls of the following Ages; viz.

	Males.	Females.	Total.		Males.	Females.	Total.
Under two years old,	31	25	56	Between 40 and 50	31	38	69
Between 2 and 5	32	36	68	50—60	28	32	60
5—10	34	38	72	60—70	20	28	48
10—20	50	51	101	70—80	7	10	17
20—30	44	63	107	80—90	2	4	6
30—40	61	62	123	90—100	0	1	1
				Total of all Ages,	339	389	728

In 1702 there were only eleven children baptized, six of whom are now living in the parish, and have resided here almost all the time.

# Account of the Inhabitants of Rome, from 1762 to 1771.

[ 445 ]

	1762	1763	1764	1765	1766	1767	1768	1769	1770	1771
Parish Churches,	81	81	81	81	82	82	82	82	82	82
Families,	35739	35696	35453	25771	35894	56375	36409	36521	37449	37285
Bishops,	42	62	45	45	51	52	54	47	52	62
Priests,	2742	2699	2718	2617	2531	2652	2676	2819	3031	2925
Religious of Auldry Orders,	4381	4291	3588	4509	4258	4105	4310	4088	5792	5739
Nuns,	1725	1892	1661	1759	1684	1738	1709	1695	1692	1554
Collegians and scholars,	868	970	763	888	734	1153	907	1197	939	491
Cardinals courts or attendants,	812	791	765	544	827	588	491	592	72	665
Poor pensioners of the Hospital,	1050	858	1271	1725	1903	2839	2010	1970	1726	1386
Prisoners,	339	240	336	402	370	390	251	405	446	402
Males of all ages,	90239	87396	88618	87203	88280	88577	88865	88415	86610	87547
Females of all ages,	67219	71423	73286	70890	69588	71183	69982	70491	71833	72128
Above 14 years of age,	120696	123211	125391	120300	119661	122150	120820	121455	120385	119984
Under 14,	36762	35608	36508	37795	38207	37610	38027	37451	38058	36991
Nonconformists to the church of Rome,	37	61	75	86	120	49	65	77	84	91
Blacks,	9	11	8	8	12	5	10	9	5	5
Devotees,	18	30	28	31	23	22	20	25	20	20
Births,	4989	5336	5420	4828	4962	4310	4595	4891	4567	4216
Deaths,	7149	6493	7361	8375	7722	7528	9574	6972	6646	5850
Total of inhabitants,	157458	158819	161899	158095	157868	159760	158847	158006	158410	156675

XLIII. *Experiments on Animals and Vegetables, with respect to the Power of producing Heat.* By John Hunter, F. R. S.

Redde, June 22, 1775. **T**HE ingenious experiments and observations lately presented to this learned Society, upon a power which animals seem to possess of generating cold, induced me to look over my notes of certain experiments and observations made in the year 1766, which indicate an opposite power in animals; whereby they are capable of resisting any external cold while alive, by generating within themselves a degree of heat sufficient to counteract it. These experiments were not originally instituted in view of the discovery, which in the event resulted from them, but for a very different purpose; which was no other than to satisfy myself, whether an animal could retain life after it was frozen, as had been confidently asserted both of fish and snakes. I mention this, to account for what might otherwise be attributed to negligence and inattention; namely, that little nicety was used in measuring the precise degrees of the cold applied in these experiments. Accuracy in this particular was not aimed at, being of no consequence in the

6

inquiry.

inquiry more immediately before me. The cold produced was first by means of ice and snow with *sal ammoniac* or sea-salt, and was about  $10^{\circ}$  of FAHRENHEIT'S thermometer. Then ice, so cooled, was mixed with spirit of nitre; but what degree of cold was thus produced I did not examine. This cold mixture was made in a tub surrounded with woollen cloths, and covered with the same, to prevent the effects of the heat of the atmosphere upon the mixture itself, and to preserve as much as possible a cold atmosphere within the vessel. The animal juices, the blood for example, freeze at  $25^{\circ}$ ; so that a piece of dead flesh could be frozen in such an atmosphere.

#### EXPERIMENTS.

I. The first experiment was made on two carp. They were put into a glass vessel with common river water, and the vessel put into the freezing mixture; the water did not freeze fast enough; and therefore, to make it freeze sooner, we put in as much cooled snow as to make the whole thick. The snow round the carp melted; we put in more fresh snow, which melted also; and this was repeated several times, till we grew tired, and at last left them covered up in the yard, to freeze by the joint operation of the surrounding mixture and the natural cold of the atmosphere. They were frozen at last, after having exhausted the whole powers of life in the production of heat. That this was really the case, could not be known, till I had completed that part of the experiment.

riment, for which the whole was begun; *viz.* the thawing of the animals. This was done very gradually; but the animals did not with flexibility recover life. While in this cold, they shewed signs of great uneasiness by their violent motions. In some of these experiments, where air was made the conductor of the cold and heat, that the heat might be more readily carried off from the animal, a leaden vessel was used. It was small for the same reason; and as it was necessary, for the animal's respiration, that the mouth of the vessel should communicate with the open air, it was made pretty deep, that the cold of the atmosphere round the animal might not be diminished fast by the warmth of the open air, which would have spoiled it as a conductor.

II. The second experiment was upon a dormouse. The vessel was sunk in the cold mixture almost to its edge. The atmosphere round the animal soon cooled; its breath froze as it came from the mouth; an hoar-frost gathered on its whiskers, and on all the inside of the vessel; and the external ends of the hair became covered with the same. While this was going on, the animal shewed signs of great uneasiness: sometimes it would coil itself into a round form, to preserve its extremities, and confine its heat; but finding that ineffectual, it then endeavoured to make its escape<sup>(a)</sup>; its motions became less violent by

(a) This shews, that cold; carried to a great degree, rather rouses the animal into action than depresses it; but it would appear, from many circumstances and observations, that a certain degree of cold produces inactivity both in the living and sensitive principle, which will be farther illustrated hereafter.

the sinking of the vital powers; and its feet were frozen; but we were not able to keep up the cold a sufficient time to freeze the whole animal, its hair being such a bad conductor of heat, that the consumption was not more, than the animal powers were capable of supporting <sup>(b)</sup>.

III. The third experiment was made upon another dormouse. From the failure of the last experiment, I took care that the hair should not a second time be an obstruction to the success of our experiment. I therefore first wetted it all over, that the heat of the animal might be more instantaneously carried off; and then it was put into the leaden vessel. The whole was put into the cold mixture as before. The animal soon gave signs of its feeling the cold, by repeated attempts to make its escape. The breath, and the evaporating water from its body were soon frozen, and appeared like a hoar-frost on the sides of the vessel, and on its whiskers; but while the vigour of life lasted, it defied the approach of the cold. However, from the hair being wet, and thereby rendered a good conductor of heat, there was a much greater consumption of it than in the former experiment. This hastened on a diminution of the power of producing it. The animal died, and soon became stiff; upon thawing it, we found it was dead.

(b) These experiments were made in presence of Dr. GEORGE FORDYCE and Dr. ERWIN, teacher of Chemistry at Glasgow; the latter of whom came in accidentally in the middle of our operations.

IV. The fourth experiment was upon a toad. It was put into water just deep enough not to cover its mouth, and the whole was put into the cold mixture, now between  $10^{\circ}$  and  $15^{\circ}$ . It allowed the water to freeze close to it, which as it were closed it in; but the animal did not die, and therefore was not frozen: however, it hardly ever recovered the use of its limbs.

V. The fifth experiment was with a snail, which froze very soon, in a cold between  $10^{\circ}$  and  $13^{\circ}$ ; but this experiment was made in the winter, when the living powers of those animals are very weak: it might have resisted the cold more strongly in the summer.

To ascertain whether vegetables could be frozen, and afterwards retain all their properties when thawed, or had the same power of generating heat with animals, I made several experiments. Vegetable juices when squeezed out of a green plant, such as cabbage and spinach, froze in a cold about  $29^{\circ}$ ; and between  $29^{\circ}$  and  $30^{\circ}$  thawed again, which is about  $4^{\circ}$  above the point at which the animal juices freeze and thaw.

I. I took a young growing bean, about three inches long in the stalk, and put it into the leaden vessel with common water, and then immersed the whole into the cold mixture. The water very soon froze all round it; however, the bean itself took up a longer time in freezing than the same quantity of water would have done; yet it did freeze, and was afterwards thawed, and planted in the ground, but it soon withered. The same experi-

riment was made upon the bulbous roots of tulips, and with the same success.

II. A young Scotch fir, which had two compleat shoots and a third growing, and which consequently was in its third year, was put into the cold mixture which was between  $15^{\circ}$  and  $17^{\circ}$ . The last shoot froze with great difficulty, which appeared to be owing in some measure to the repulsion between the plant and the water. When thawed, the young shoot was found flaccid. It was planted; the first and second shoot we found retained life, while the third, or growing shoot, withered.

III. A young shoot of growing oats with three leaves, had one of the leaves put into the cold mixture at  $22^{\circ}$ , and it soon was frozen. The roots were next put in, but did not freeze; and when put into the ground, the whole grew, excepting the leaf which had been frozen. The same experiment was made upon the leaves and roots of a young bean, and attended with the same success.

IV. A leaf taken from a growing bean was put into the cold mixture, and frozen, and afterwards thawed, which served as a standard. Another fresh leaf was taken and bent in the middle upon itself; a small shallow leaden vessel was put upon the top of the cold mixture, and the two leaves put upon its bottom; but one-half of each leaf was not allowed to touch the vessel by the bend; the cold mixture was between  $17^{\circ}$  and  $15^{\circ}$ , and the atmosphere at  $22^{\circ}$ . The surfaces of the two leaves which were in contact with the lead were soon frozen.



frozen in both; but those surfaces which rose at right angles, and were therefore only in contact with the cold atmosphere, did not freeze in equal times; the one that had gone through this process before, froze much sooner than the fresh one. The above experiment was repeated when the cold mixture was at  $25^{\circ}$ ,  $24^{\circ}$ , and the atmosphere nearly the same, and with the same success; only the leaves were longer in freezing, especially the fresh leaf.

V. The vegetable juices above mentioned being frozen in the leaden vessel, the cold mixture at  $28^{\circ}$ , and the atmosphere the same, a growing fir-shoot was laid upon the surface, also a bean-leaf; and upon remaining there some minutes, they were found to have thawed the surface on which they lay. This I thought might arise from the greater warmth of these substances at the time of application; but by moving the fir-shoot to another part, we had the same effect produced.

VI. A fresh leaf of a bean was exactly weighed; it was then put into the cold atmosphere and frozen. In this state it was put back into the same scale, and allowed to thaw. No alteration in the weight was produced.

It appears from the above experiments, that an animal must be deprived of life before it can be frozen. Secondly, that there is an exertion, or an expence of animal powers, in doing this, in proportion to the necessity; and that the whole animal life may be exhausted in this way. Thirdly, that this power is in proportion to the

the

the perfection of the animal, the natural heat proper to each species, and to each age. It may also perhaps depend, in some degree, on other circumstances not hitherto observed: for from experiment II. and III. upon dormice, I found that in these animals, which are of a constitution to retain nearly the same heat in all temperatures of the air, it required the greatest cold I could produce to overcome this power; while in experiment IV. and V. this power in the toad and snail, whose natural heat is not always the same, but is altered very materially according to the external heat or cold, was exhausted in a degree of cold not exceeding  $10^{\circ}$  or  $15^{\circ}$ : and the snail being the most imperfect of the two, its powers of generating heat were by much the weakest.

That the imperfect animals will allow of a considerable variation in their temperature of heat and cold, is proved by the following experiments. The thermometer being at  $45^{\circ}$ , having introduced the ball by the mouth into the stomach of a frog, which had been exposed to the same cold, it rose to  $49^{\circ}$ . I then put the frog into an atmosphere made warm by heated water, and allowed it to stay there twenty minutes; when, upon introducing the thermometer into the stomach, it raised the quicksilver to  $64^{\circ}$ . But to what degree the more imperfect animals are capable of being rendered hotter and colder, at one time than another, I have not been able to determine. The torpidity of these animals in our winter is probably owing to the great change wrought in their temperature

by the external heat and cold. The cold in their bodies is carried to such a degree, as in great measure to put a stop, while it lasts, to the vital functions. In warmer climates no such effect is produced. In this respect they resemble vegetables.

From the foregoing experiments it appears; first, that plants when in a state of actual vegetation, or even in such a state as to be capable of vegetating under certain circumstances, must be deprived of their principle of vegetation before they can be frozen. Secondly, Vegetables have a power within themselves of producing or generating heat; but not always in proportion to the diminution of heat by application of cold, so as to retain at all times an uniform degree of heat: for the internal temperature of vegetables is susceptible of variations to a much greater extent indeed than that of the more imperfect animals; but still within certain limits. Beyond these limits the principle of vegetable, as of animal life, resists any further change. Thirdly, the heat of vegetables varies, according to the temperature of the medium in which they are, which we discover by varying that temperature, and observing the heat of the vegetable. Fourthly, the expence of the vegetating powers in this case is proportioned to the necessity, and the whole vegetable powers may be exhausted in this way. Fifthly, this power is most probably in proportion to the perfection of the plant, the natural heat proper to each species, and the age of each individual. It may also perhaps depend, in some degree, on other circumstances not hitherto observed; for in experiment

periment II. the old shoot did not lose its powers, while that which was young or growing did; and in experiment III. and IV. we found, that the young growing shoot of the fir was with great difficulty frozen at  $10^{\circ}$ , while a bean-leaf was easily frozen at  $22^{\circ}$ ; and in experiment V. the young shoot of the fir thawed the ice at  $28^{\circ}$ , much faster than the leaf of the bean. Sixthly, it is probably, by means of this principle, that vegetables are adapted to different climates. Seventhly, that suspension of the functions of vegetable life, which takes place during the winter season, is probably owing to their being susceptible of such a great variation of internal temperature. Eighthly, the roots of vegetables are capable of resisting cold more than the stem or leaf; therefore, though the stem be killed by cold, the root may be preserved, as daily experience evinces. The texture of vegetables alters very much by the loss of life, especially those which are watry and young; from being brittle and crisp they become tough and flexible. The leaf of a bean when in full health is thick and massy, repels water as if greasy, and will often break before it is considerably bent; but if it is killed slowly by cold, it will lose all these properties, becoming then pliable and flaccid; deprived of its power of repelling water, it is easily made wet, and appears like boiled greens. If killed quickly, by being frozen immediately, it will remain in the same state as when alive; but upon thawing, will immediately lose all its former texture. This is so

remarkable, that it would induce one to believe, that it lost considerably of its substance; but from experiment vi. it is evident that it does not. The same thing happens to a plant when killed by electricity (c). If a growing juicy plant receives a stroke of electricity sufficient to kill it, its leaves droop, and the whole becomes flexible.

So far animal and vegetable life appear to be the same; yet an animal and a vegetable differ in one very material circumstance, which it may be proper to take particular notice of in this place, as it shews itself with remarkable evidence in these experiments. An animal is equally old in all its parts, excepting where new parts are formed in consequence of diseases; and we find, that these new or young parts in animals, like the young shoots of vegetables, are not able to support life equally with the old; but every plant has in it a series of ages. According to its years, it has parts of all the successive ages from its first formation; each part having powers equal to its age, and each part, in this respect, being similar to animals of so many different ages. Youth in all cases is a state of imperfection; for we find that few animals that come into the world in winter live, unless they are particularly taken care of; and we may observe the same of vegetables. I found that a young plant

(c) To kill a whole plant by electricity, it is necessary to apply the conductor, or give a shock to every projecting part; for any part that is out of the line of direction will still retain life.

was more easily killed than an old one; as also the youngest part of the same plant.

This power of generating heat seems to be peculiar to animals and vegetables while alive. It is in both a power only of opposition and resistance; for it is not found to exert itself spontaneously and unprovoked; but must always be excited by the energy of some external frigorific agent. In animals it does not depend on the motion of the blood, as some have supposed, because it belongs to animals who have no circulation; besides, the nose of a dog, which is nearly always of the same heat in all temperatures of the air, is well supplied with blood: nor can it be said to depend upon the nervous system, for it is found in animals that have neither brain or nerves. It is then most probable, that it depends on some other principle peculiar to both, and which is one of the properties of life; which can, and does, act independently of circulation, sensation, and volition; *viz.* that power which preserves and regulates the internal machine, and which appears to be common to animals and vegetables. This principle is in the most perfect state when the body is in health, and in many deviations from that state, we find that its action is extremely uncertain and irregular; sometimes rising higher than the standard, and at other times falling much below it. Instances of this we have in different diseases, and even in the same disease, in very short intervals of time. A very remarkable one fell under my own observation, in a gentleman who was taken with an

apoplectic fit; while he lay insensible in bed, and covered with blankets, I found that his whole body would, in an instant, become extremely cold in every part; continue so for some time; and, in as short a time, he would become extremely hot. While this was going on for several hours alternately, there was no sensible alteration in his pulse.

XLIV. *A Comparison of the Heat of London and Edinburgh.* By John Roebuck, M. D. F. R. S. in a Letter to William Heberden, M. D. F. R. S.

S I R,

Redde, June 29, 1775. **I** DELIVERED to you some time ago, a register of the thermometer at Hawkhill for ten years; but as these observations were made at eight o'clock in the morning and four in the afternoon, and yours at eight o'clock in the morning and two in the afternoon, the corresponding years of the morning's observations only admit of a comparison. It appears by your register, that the mean heat at London for nine years, from the end of 1763 to the end of 1772, at eight o'clock in the morning, was  $47.4^{\circ}$ ; and the mean heat at Hawkhill, during the same period of time, was  $46^{\circ}$ . The difference of which is only  $1.4^{\circ}$ . A difference much less than might be expected from the difference of latitude, and not sufficient to account why nonpareils, golden rennets, peaches, nectarines, and many kinds of grapes, generally come to maturity near London, and scarce ever near Edinburgh, without the aid of artificial heat. Before I proceeded further to perplex myself with this difficulty,



ficulty, I procured from Hawkhill and from yourself the register of the thermometer for three years, at the same periods of time; copies of which I here inclose you. And by these it appears, that the mean heat of London of these three years exceeded that of Edinburgh by  $4.5^{\circ}$ . And the mean heat of the three hottest months in London exceeded the mean of the same three at Edinburgh by  $5.8^{\circ}$ . And the mean heat of these three summer months, at two o'clock in the afternoon, in London exceeded the mean heat of the same months, at the same hour, in Edinburgh by  $7.3^{\circ}$ ; which sufficiently accounts why some fruit may come to maturity in one country and not in the other: and also why corn and grass, which vegetate with a more temperate heat, but require a longer continuance of it, may arrive at maturity in both countries. The reason why the mean heat of London exceeds that of Edinburgh may arise principally from the difference of latitude. But the reason why the excess is greater in proportion in the three hottest months of the year, at the hottest time of the day, than in the winter months, arises from Edinburgh's being situated nearer to the sea than London. We might speak with more precision on this subject, if we had a register of the thermometer at Moscow, which is nearly of the same latitude as Edinburgh; though it is well known, that the heat of summer is much more intense, and the cold of winter much more severe, at Moscow than at Edinburgh. The mean heat of springs near Edinburgh seems to be  $47^{\circ}$ ; and at London

don  $51^{\circ}$ . It is probable, that the mean heat of good springs in any country is very nearly the mean heat of the country <sup>(a)</sup>. A faithful account of the heat of springs in different latitudes, and of water taken from the same depth of the sea in different latitudes is yet wanted.

I am, &c.

(a) We shall have an easy method of finding the mean heat of any place, if it be always nearly equal to that of its springs. This matter might be ascertained by a proper number of observations; and it is therefore very desirable, to have an account taken of the heat of the springs, wherever a register is kept of the heat of the air. W. HEBERDEN.

Mean Heat in PALL MALL, LONDON.								
	1772.		1773.		1774.		Mean heat of Three Years.	
	8 A.M.	2 P.M.	8 A.M.	2 P.M.	8 A.M.	2 P.M.	8 A.M.	2 P.M.
January	36	38	42	44	34	39	37.3	40.3
February	38	42	36	41	38	44	37.3	42.3
March	41	47	40	51	41	52	40.7	50
April	44	51	45	55	47	55	45.3	53.7
May	49	60	50	60	51	60	50	60
June	64	73	58	67	59	67	60.3	69
July	61	72	60	68	61	69	60.7	69.7
August	60	70	62	72	62	70	61.3	70.7
September	56	65	56	63	55	63	55.7	63.7
October	56	61	51	59	48	58	51.7	59.3
November	45	55	40	47	40	44	41.7	48.7
December	41	44	41	45	39	43	40.3	44
Mean	49.2	56.5	48.4	56	47.9	55.3	48.5	56
Mean heat of three years morning and afternoon was 52.2.								

Mean

Mean heat at HAWKHILL, situated about one mile North of Edinburgh, and 103 feet above the level of the sea.

	1772.		1773.		1774.		Mean Heat of Three Years.	
	8 A.M.	2 P.M.	8 A.M.	2 P.M.	8 A.M.	2 P.M.	8 A.M.	2 P.M.
January	31.5	34.3	38.5	40.3	29.1	33	33.3	35.8
February	30.9	36.5	35.1	40.7	36.2	40.4	34	39.2
March	37	42.8	42.1	48.4	37.1	43.2	38.7	44.8
April	42.9	48.5	45.6	51.1	44.1	48.9	44.2	49.5
May	49.1	54.5	48.6	53.1	46.6	50.8	48.1	52.8
June	57.2	62.1	55.2	60.1	51.1	59.7	54.5	60.6
July	58.7	64.6	57.7	61.9	57.4	63.3	57.9	63.3
August	57.4	63.9	58.3	64.8	57.2	62.5	57.6	63.7
September	51.5	58.1	51.3	55.8	51.7	57.8	51.5	57.2
October	48.8	51.6	46	50.7	48.3	52.8	47.7	51.7
November	41.7	44.6	38.2	42.3	38	42	39.3	42.9
December	39.7	41.6	36.4	38.5	37.3	40	37.8	40
Mean heat,	45.5	50.3	46.1	50.6	44.5	49.5	45.4	50.1

Mean heat of three years morning and afternoon was 47.7.

XLV. *Experiments in an heated Room.* By Matthew Dobson, M. D. *In a Letter to John Fothergill, M. D. F. R. S.*

DEAR SIR,

Liverpool, April 25, 1775.

Redde, June 22,  
1775.

I PERUSED with particular pleasure, your short account of the curious experiment made by Mr. BANKS and Dr. SOLANDER. The same, and some additional experiments, have been made here; the result of which I should sooner have transmitted to you, had I not been prevented by the constant engagements of my profession.

#### EXPERIMENTS.

I. The sweating-room of our Public Hospital at LIVERPOOL, which is nearly a cube of nine feet, lighted from the top, was heated till the quicksilver stood at  $224^{\circ}$  on FAHRENHEIT's scale, nor would the tube of the thermometer indeed admit the heat to be raised higher. The thermometer was suspended by a string fixed to the wooden frame of the sky-light, and hung down about the centre of the room. Myself and several others were at this time inclosed in the stove, without experiencing any oppressive or painful sensation of heat, proportioned to the degree pointed out by the thermometer. Every metallic about us soon became very hot.

VOL. LXV.

Q q q

II. My

II. My friend Mr. PARK, an ingenious surgeon of this place, went into the stove heated to  $202^{\circ}$ . After ten minutes, I found the pulse quickened to  $120^{\circ}$ . And to determine the increase of the animal heat, another thermometer was handed to him, in which the quicksilver already stood at  $98^{\circ}$ ; but it rose only to  $99\frac{1}{2}$ , whether the bulb of the thermometer was inclosed in the palms of the hands, or received into the mouth<sup>(a)</sup>. The natural state of this gentleman's pulse is about 65.

III. Another gentleman went through the same experiment in the same circumstances, and with the same effects.

IV. One of the porters to the Hospital, a healthy young man, and the pulse 75, was inclosed in the stove when the quicksilver stood at  $210^{\circ}$ ; and he remained there, with little inconvenience, for twenty minutes. The pulse, now 164, and the animal heat, determined by another thermometer as in the former experiments, was  $101\frac{1}{2}$ .

V. A young gentleman of a delicate and irritable habit, whose natural pulse is about 80, remained in the stove ten minutes when heated to  $224^{\circ}$ . The pulse rose to 145, and the animal heat to  $102^{\circ}$ . This gentleman, who had been frequently in the stove during the course of the day, found himself feeble, and disposed to break out into sweats for 24 hours after the experiment.

(a) The scale of the thermometer, which was suspended by the string about the middle of the room, was of metal; this was the only one I could then procure, on which the degrees ran so high as to give any scope to the experiment. The scale of the other thermometer, which was employed for ascertaining the variations in the animal heat, was of ivory.

VI. Two small tin vessels, containing each the white of an egg, were put into the stove heated to  $224^{\circ}$ . One of them was placed on a wooden seat near the wall, and the other suspended by a string about the middle of the stove. After ten minutes, they began to coagulate; but the coagulation was sensibly quicker and firmer in that which was suspended, than in that which was placed on the wooden seat. The progress of the coagulation was as follows: it was first formed on the sides, and gradually extended itself; the whole of the bottom was next coagulated; and last of all the middle part of the top.

VII. Part of the shell of an egg was peeled away, leaving only the film which surrounds the white; and part of the white being drawn out, the film sunk so as to form a little cup. This cup was filled with some of the *albumen ovi*, which was consequently detached as much as possible from every thing but the contact of the air and of the film which formed the cup. The lower part of the egg stood upon some light tow in a common gallipot, and was placed on the wooden seat in the stove. The quicksilver in the thermometer still continued at  $224^{\circ}$ . After remaining in the stove for an hour, the lower part of the egg which was covered with the shell, was firmly coagulated; but that which was in the little cup was fluid and transparent. At the end of another hour it was still fluid, except on the edges where it was thinnest; and here it was still transparent; a sufficient proof that it was dried, not coagulated.

VIII. A piece of bees wax, placed in the same situation with the *albumen ovi* of the preceding experiment, and exposed to the same degree of heat in the stove, began to melt in five minutes: another piece suspended by a string, and a third piece put into the tin vessel and suspended, began likewise to liquify in five minutes.

## OBSERVATIONS.

That heated air should have such a speedy and powerful effect in quickening the pulse, while the animal heat is little altered from its natural standard; that the human body should so easily bear to be surrounded with air heated to  $224^{\circ}$ ; that the *albumen ovi*, which begins to coagulate in water at  $150^{\circ}$ , should remain fluid in  $224^{\circ}$ ; and that the same *albumen ovi*, still placed in air heated to  $224^{\circ}$ , should coagulate if in contact either with tin or its own shell, are facts as singular as they are difficult of explanation. From the different effects of heated air on the pulse and the heat of the body, do we not discover the fallacy of that theory of animal heat which has been adopted by BOERHAAVE and other celebrated physiologists? They suppose that animal heat is produced by the attrition of the *globules* of the circulating fluids against the sides of the containing vessels; but in several of the preceding experiments, the circulation was amazingly quickened with little increase of the animal heat. But whence is it that the human body can bear without immediate injury, to be surrounded with air heated to  $224^{\circ}$ ? And whence is it, that the *albumen ovi* does not coagulate

in

in this degree of heat? Is it that fire as it passes into some bodies becomes latent, agreeable to a doctrine which has for some time been taught at Edinburgh by Professor BLACK? Or does fire become fixed and *quiescent*, according to a similar system adopted by Dr. FRANKLIN<sup>(b)</sup>? Air we know exists either in a fixed or elastic state; and fire may in like manner exist in bodies, either in a latent, fixed, and *quiescent*; or in a sensible, fluid, and active state. Agreeable to this idea, the bees wax receives the fire in an active state, and dissolves; while the human body and the *albumen ovi*, receiving the fire in a latent state, are little altered in their temperature. Let each of these, however, be put in contact with a different body, tin for instance; and though the heat of the air continues the same, yet the fire no longer enters in a latent state, but with all its sensible and active powers; for the *albumen ovi* suspended in a tin vessel soon coagulates; and the human body, covered with the same metal, would quickly experience an intolerable and destructive degree of heat. Or are the above phenomena more satisfactorily explained, by considering different bodies as possessing different conducting powers; some being strong, others weak conductors of fire? All those bodies then which are weak conductors of fire from air, may be placed in air, without receiving the heat of this medium. Hence the *albumen ovi* remains fluid in air heated to 224°. Hence likewise the frog, the lizard, the camelion, &c. retain their natural temperature, and feel cold

(b) Exper. and Observ. p. 346. and 412.



to the touch, though perpetually furrounded with air hotter than their own bodies. Hence altho, the human body keeps nearly its own temperature, in a stove heated to  $224^{\circ}$ : or may even pass without injury into air heated to a much greater degree, according to the observations of DU HAMEL and TILLET, published in the Memoirs of the Academy of Sciences (c). On the other hand, all those bodies which are powerful conductors of fire from air, are influenced in proportion when furrounded with this medium. The bees wax melted from the mere contact of the air in experiment VIII; and in experiment VI, the *albumen ovi* was coagulated on the intervention of another body, which is a strong conductor of fire from air. But whether this method of reasoning on the natural cause of these effects be just or not, the final cause is obvious, and is to be resolved into the wise and benevolent appointment of the Almighty. Man is happily so framed, as to possess a power of keeping nearly the same tenor of heat, in all the variations of the temperature of the air in summer and in winter, in hot and cold climates; and consequently changes his situation on the surface of the globe, with much less inconvenience or injury, than he could otherwise have done. The same power likewise happily adapts different animals to their respective destinations. The lizard and the camelion remain cool under the Equator, while the whale and porpoise retain a degree of heat above that of the human body, though furrounded with the waters of the coldest

(c) Memoires pour 1761.

Northern seas, and amidst mountains of ice in the neighbourhood of the Pole.

Should you think these experiments and observations on heated air of sufficient importance to be communicated to the Royal Society, they are at your disposal.

I have the pleasure to find, that Dr. PRIESTLEY is prosecuting his very ingenious inquiries on air. In a letter I lately received from him, he informs me, that he has discovered a species of air, which will preserve animal life six times longer than atmospheric air.

I remain, with great esteem, &c.

XLVI. *Calculations in Spherical Trigonometry abridged.*

*By Israel Lyons. In a Letter to Sir John Pringle,  
Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

SIR,

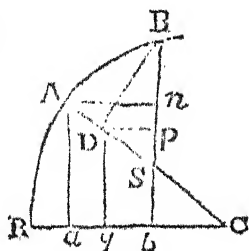
Redde, July 6, <sup>1775.</sup> SINCE astronomical observations have been made with much greater precision than formerly, it became requisite that the calculations corresponding to them should likewise be made to much greater degrees of exactness. The ancient astronomers desired only to make their observations and computations agree within a part of a degree; succeeding ones were satisfied when they corresponded within a minute; but no less exactness than seconds will content the moderns. The rules in spherical trigonometry being reduced to operations by logarithms, it is necessary to use such a number of figures in the tables as will produce the required precision; this is very different in the various parts of the quadrant, insomuch that if the arc is only one degree, four places of decimals in the logarithm of a sine are sufficient to determine the arc to which it belongs within a second: whereas if the arc is  $89^{\circ}$ , there is a necessity

necessity of using eight figures for the same purpose: thus, the logarithm sine of  $89^{\circ} 0' 0''$  is 9.9999338, the same seven figures as for the logarithm sine of  $89^{\circ} 0' 1''$ . From this consideration it follows, that the analogies commonly laid down and used for the solutions of spherical triangles are not in all cases equally convenient, and I might say, equally accurate; and that it would be more easy and exact in calculations to find what was required, by means of sines of arcs, which, being small, require the use of only a few places of figures. Now the cases which often occur in astronomy, where spherical trigonometry can only be of use, are generally of such a nature that we know nearly, or at least within a few degrees, what the required side or angle is, there is nothing therefore wanted but to find how much this quantity, or first approximation, differs from the true value of the side or angle. Thus in calculating the right ascension of any point of the ecliptic, whose longitude and declination are known, instead of finding the right ascension immediately, it will be more convenient to seek for the difference between the longitude and right ascension, which as it never exceeds  $2\frac{1}{2}^{\circ}$ , four or five places of figures will always be sufficient to determine it within a second. And in other similar cases, rules might be made agreeable to the exigency of each particular case, which would be better than the application of the general method of solution. Some examples of which shall be shewn in the

following paper: the design of which is to point out a method of solving several of the most useful questions in spherical trigonometry in a manner somewhat similar to that used in approximating to the roots of algebraic equations. This method is founded on the following

L E M M A.

If the radius is supposed equal to unity, the sine of the sum of two arcs,  $\alpha$  and  $\beta$ , is equal to  $\sin. \alpha + \cos. \alpha \times \sin. \beta - \sin. \alpha \times \text{vers.} \sin. \beta$ . And its cosine  $= \cos. \alpha - \sin. \alpha \times \sin. \beta - \cos. \alpha \times \text{vers.} \sin. \beta$ .



D E M O N S T R A T I O N .

Let the arc  $\alpha$  be RA, and the arc  $\beta$  be AB, their sines Aa, BD, respectively; then Bb being drawn perpendicular to the radius CR will be the sine of  $\alpha + \beta$ . Draw Dp and An parallel to CR. Then, by similar triangles, CA : Ca :: BD : Bp, and CA : Aa :: AD : np. Therefore,  $Bb (= Aa + Bp - pn) = Aa + \frac{Ca \times BD}{CA} - \frac{Aa \times AD}{CA}$ ; that is,  $\sin. \alpha + \beta = \sin. \alpha + \cos. \alpha \times \sin. \beta - \sin. \alpha \times \text{vers.} \sin. \beta$ .

In the same manner, drawing Dq parallel to Aa we may prove  $cb (= Ca - bq - aq) = CA - \frac{Aa \times BD}{CA} - \frac{Ca \times AD}{CA}$ , or  $\cos. \alpha + \beta = \cos. \alpha - \sin. \alpha \times \sin. \beta - \cos. \alpha \times \text{vers.} \sin. \beta$ .

In

In what follows, for brevity sake, the arc is expressed by a Greek letter; its sine by the capital character; and the cosine by the small italic character of the same letter. In this notation, the two theorems will stand thus,  $\sin. \overline{\alpha + \beta} = A + aB - A \times \text{vf. } \beta$ , and  $\cos. \overline{\alpha + \beta} = a - AB - a \times \text{vf. } \beta$ .

# COROLLARY I.

Since the tangent is equal to the sine divided by the cosine, we shall have

$$\text{Tang. } \overline{\alpha + \beta} = \frac{A + aB - A \times \text{vf. } \beta}{a - AB - a \times \text{vf. } \beta} = \frac{A}{a} + \frac{B}{a^2} + \frac{A}{a^3} \times \text{vf. } \beta \text{ nearly.}$$

# COROLLARY II.

If we change the sign of  $\beta$ , we shall have  $\sin. \alpha - \beta = A - aB - A \times \text{vf. } \beta$ .  $\cos. \alpha - \beta = a + AB - a \times \text{vf. } \beta$ . And  $\text{tang. } \alpha - \beta = \frac{A}{a} - \frac{B}{a^2} + \frac{A}{a^3} \times \text{vf. } \beta$ .

By the help of these theorems, knowing nearly what any quantity in a spherical triangle is, we may find its correction, thus: if we have to find the cosine of an arc, which arc we know is nearly equal to  $a$  whose cosine is  $a$ . Suppose the arc to be  $\alpha - \beta$ , and its cosine  $a + c$ . Then  $a + c = \cos. \alpha - \beta = a + AB - a \times \text{vf. } \beta$ . Therefore,  $B = \frac{c}{A} + \frac{a}{A} \times \text{versf. } \beta$ .

The first term  $\frac{c}{A}$  will always give a near approximation to the value of  $\sin. \beta$ , and  $\beta$  being found the correction,  $\frac{a}{A} \times \text{vf. } \beta$ , or  $\cot. \alpha \times \text{vf. } \beta$ , may be found and added to it.

Among the tables requisite to be used with the Nautical Almanac, is table iv. for parallax, p. 19. which shews the value at sight of such quantities as  $\text{vf. } \beta \times \cot. \alpha$ , the arc  $\beta$  being found in the first column of the table, and  $\alpha$  at the top. This table I have calculated only to arcs under  $63'$ ; but it would be found useful to have a table ready computed for all arcs under  $5^\circ$ .

P R O B L E M I.

If the two legs, AB and BC, of the spherical triangle ABC right-angled at B, are given, to find the hypotenuse AC, the leg BC, being small in comparison of AC.



Let  $AB = \alpha$ ,  $BC = \beta$ , and suppose  $AC = \alpha + \zeta$ ,  $\alpha$  being a near approximation to AC, and  $\zeta$  the small arc to be added to AB to make it equal to AC; then  $\cos. AC = \cos. AB \times \cos. BC$ ; that is, according to our notation,  $a - AZ = \overline{a \times \text{vf. } \zeta} = ab$ .

Whence  $Z = \frac{a-ab}{A} - \frac{a}{A} \times \text{vf. } \zeta = \cot. \alpha \times \text{vf. } \beta - \cot. \alpha \times \text{vf. } \zeta$ .

E X A M P L E.

Let AB be  $75^\circ 0'$  and BC  $20^\circ 0'$ , and the computation will be as follows:

Cotangent AB.	9.4280
Verfed sine BC	8.7804

---

$\zeta$ nearly	$55' 33''$ sine	8.2084
----------------	-----------------	--------

Correction	$-7$ from tab. IV. Nautical Almanac.
------------	--------------------------------------

---

Therefore  $\zeta = 55' 26''$  and  $AC = 75^\circ 55' 26''$ .

By this problem, the distance of the Sun may be found from a planet whose latitude and difference of longitude are known.



P R O B L E M II.

Having the hypotenuse,  $AC$  and one of the angles  $A$ , to find the base  $AB$ .

Let  $AC = \beta$ ,  $BAC = \alpha$ , and suppose  $AB = \beta - \zeta$ , then  $\cos. A = \cot. AC \times \tan. AB$ , or  $a = \frac{b}{B} \times \frac{B}{b} - \frac{z}{b^2} + \frac{B \times \text{vf. } \zeta}{b^3} = 1 - \frac{z}{Bb} + \frac{B \times \text{vf. } \zeta}{b^2}$ .  
Whence  $z = Bb \times 1 - a + \frac{B}{b} \times \text{vf. } \zeta = \frac{1}{2} \sin. 2\beta \times \text{vf. } \alpha + \tan. \beta \times \text{vf. } \zeta$ .

E X A M P L E.

Let  $A = 23^\circ 28' 15''$ , and  $AC = 10^\circ 0' 0''$ .

Sine  $2AC$   $20^\circ 0'$  9.5340

Verfed sine  $A$  8.9177

---

8.4517

Log. 2. 0.3010

---

$\zeta$  nearly 48' 39" sine 8.1507

Correction + 6

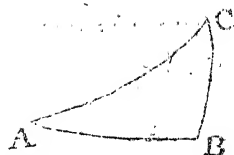
---

$\zeta$  48 45 and  $BC = 159^\circ 11' 15''$ .

By this problem, the right ascension of any point of the ecliptic, whose obliquity and longitude are known, may be found.

P R O B L E M III.

*Supposing the same things known as in the last, to find the perpendicular BC, when the hypotenuse is nearly a quadrant.*



Let  $A = \alpha$ ,  $AC = \beta$ , as before, and suppose  $BC = a - \zeta$ ; then  
 $\sin. BC = \sin. AC \times \sin. A$ , or  $A - aZ - A \times \text{vf. } \zeta = AB$ , whence  

$$x = \frac{A - BA}{a} - \frac{A}{a} \times \text{vf. } \zeta = \text{tang. } \alpha \times \text{co. ver. fin. } \beta - t. \alpha \times \text{vf. } \zeta.$$

E X A M P L E.

Let  $A = 23^\circ 28' 15''$ , and  $AC = 80^\circ 0'$ .

Tang. A 9.6377

Verf. fin. co. AC,  $10^\circ$  8.1816

$\zeta$  nearly 22° 41 fine 7.8193

Correction - 1

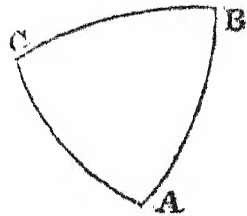
$\zeta$  22 40 and BC = 23° 5' 35''.

This problem will be of use to find the declination of the ecliptic, and the latitude of a planet near the limits.

These three instances will suffice for an application of this method to right-angled spherical triangles; we shall now give two problems of oblique triangles.

PROBLEM IV.

Suppose ABC to be a spherical triangle, in which are given the two sides AB, BC, with the included angle B, to find the third side AC.



SOLUTION I.

Let  $ABC = \beta$ ,  $BC = \alpha$ ,  $AB = \delta$ . Put  $AC = \beta + \zeta$ ,  $\beta$  being an approximate value of  $AC$ , when the two legs are nearly quadrants. Now the cosine of  $AC$  being equal to  $b_{DA} + da^{(a)}$  we shall have  $b - BZ - b \times \text{vf. } \zeta = b_{DA} + da$ : and  $Z = \frac{b - b_{DA} - da}{B} - \frac{b}{B} \times \text{vf. } \zeta$ . But  $1 - DA - da = \text{vf. } \delta - \alpha$ , which put  $= w$ . Then  $Z = \frac{bw}{B} + \frac{bda - da}{B} - \frac{b}{B} \times \text{vf. } \zeta = \cot. \beta \times \text{vf. } \delta - \alpha - \cot. \delta \times \cot. \alpha \times \text{tang. } \frac{1}{2}\beta - \cot. \beta \times \text{vf. } \zeta$ . Therefore  $\zeta$  is the difference of two arcs whose sines are  $\cot. \beta \times \text{vf. } \delta - \alpha$ , and  $\cot. \delta \times \cot. \alpha \times \text{tang. } \frac{1}{2}\beta$ , the difference of these two arcs being diminished by the correction  $\cot. \beta \times \text{vf. } \zeta$ .

(a) It is a well known theor. that  $\sin. BA \times \sin. BC : r^2 = \text{vf. } AC - \text{vf. } AB - BC : \text{vf. } B$ ; that is,  $\sin. BA \times \sin. BC : r^2 = \cot. AB - BC - \cot. AC : r - \cot. B$ . Or, in the author's notation, putting  $r = 1$ ,  $DA : 1 = \cot. \delta - \alpha - \cot. AC : 1 - b$ . Therefore  $DA - b_{DA} = \cot. \delta - \alpha - \cot. AC$ . Or,  $\cot. AC = b_{DA} - DA - \cot. \delta - \alpha$ . For  $\cot. \delta - \alpha$  substitute its value as expressed in the second corollary of the lemma, and there arises the author's equation,  $\cot. AC = b_{DA} + da$ .

S. HORSLEY.

EXAMPLE.

EXAMPLE.

Suppose  $B = 51^{\circ} 12' 5''$

$AB = 87 \quad 57 \quad 51$

$BC = 87 \quad 20 \quad 34$

Cotangent B  $9.9053$

Verf. fine  $AB - BC \quad 0^{\circ} 37' 17'' \quad 5.7693$

Tang.  $\frac{1}{2} B \quad 25^{\circ} 56' \quad 9.6804$

Cofine AB  $8.5506$

Cofine BC  $8.6661$

1st arc  $0^{\circ} 10''$  fine  $5.6746$

2d arc  $2' 43''$  fine  $6.8971$

The difference of these two arcs,

$2' 33''$

Subtracted from the value of the angle B,  $51 \quad 12 \quad 5$

Leaves AC,

$51 \quad 9 \quad 32$

The correction cot.  $\beta \times \text{vf. } \zeta$  in this example is 0.

This solution is very convenient to find the distance of two Zodiacal Stars, having their latitudes and difference of longitude.

SOLUTION II.

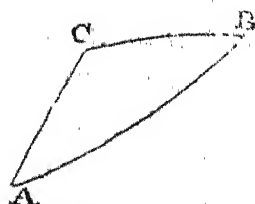
Let  $\tau$  be an arc whose cofine  $t = b \times \text{cof. } \delta - \alpha = b da + b DA$ , and suppose  $AC = \tau - \zeta$ , then  $t + TZ - t \times \text{vf. } \zeta = b DA + da = t - b da + da$ . Whence  $z = da \times \frac{1-b}{T} + \frac{t}{T} \times \text{verf. } \zeta = \text{cofec. } \tau \times \text{cofin. } \alpha \times \text{cofin. } \delta \times \text{vf. } \beta + \text{cot. } \tau \times \text{vf. } \zeta$ .

This solution is useful to find the distance of the Moon from a star at some distance from the ecliptic, in which case it coincides with the rule given by the Astronomer

Royal, Phil. Transf. 1764, vol. LIV. and which taking in the correction here given  $\cot. \tau \times \text{vf. } \zeta$  will always be exact to a second. It is also of use to find the declination of a star, whose longitude and latitude and obliquity of the ecliptic are known.

### SOLUTION III.

Let the angle B be small, and the two legs AB, BC, very unequal; then the side AC will be nearly  $AB - BC$ . Put this =  $k$ , and suppose  $AC = k + \zeta$ , then  $\cos.$



$$AC = k - kZ - k \times \text{vf. } \zeta = ad + AD - kZ - k \times \text{vf. } \zeta = bAD + ad,$$

$$\text{Whence } Z = \frac{DA - bAD}{k} - \frac{k}{k} \times \text{vf. } \zeta = \sin. \delta \times \sin. \alpha \times \text{vf. } \beta \times \text{cosec.}$$

$$\delta - \alpha - \cot. k \times \text{vf. } \zeta.$$

### EXAMPLE.

Let  $AB = 94^\circ 36' 58''$   
 $BC = 23 \quad 28 \quad 24$   
 $B = 24 \quad 54 \quad 24$  } as in the example to fol. 2.

$AB - BC = 71 \quad 8 \quad 34$	Cosecant	0.02396
Sine AB		9.99859
Sine BC		9.60023
Verfed of B		8.96851
$\zeta$ nearly $2^\circ 14' 11''$ fine		8.59129

The value of  $\zeta$  being without the limits of tab. iv. in the tables requisite to be used with the Nautical Almanac, the correction  $\cot. \mu \times \text{vf. } \zeta$  must be computed thus:

Cot. $\mu$	9.533
V. fin. $\zeta$	6.881
	<hr/>

Cor.  $0' 53''$ . sine 6.414, this subtracted from the first value of  $\zeta$ , leaves  $\zeta = 2^\circ 13' 18''$ , which added to  $\delta - \alpha$ , gives the side  $AC = 73^\circ 21' 52''$ . This solution will help to find the Sun's altitude near noon.

I have dwelt the longer on this problem because it is one that is very commonly required in astronomical calculations, and the operation by the rules of spherical trigonometry in this as well as the next is rather troublesome.

PROBLEM V.

Supposing the same things given, to find either of the angles, as for instance C opposite the side AB.

We have  $\cot. c = \cot. B \times \cos. BC - \sin. BC \times \cot. AB \times \cos. C. B$   
 $= \frac{ba}{B} - \frac{Ad}{BD}$ . Let  $\mu$  be an angle whose  $\cot. \frac{m}{M} = \cot. \beta \times$   
 $\sin. \delta - \alpha \times \cos. \delta = \frac{baD - bAd}{BD}$ , and suppose  $c = \mu + \zeta$ , then  
 $\cot c = \frac{m}{M} - \frac{z}{M^2} + \frac{m.vf.\zeta}{M^2} = \frac{baD - Ad}{BD}$ . Whence  $z = M \times \frac{Ad - baD}{BD} +$   
 $\frac{m}{M} \times vf. \zeta = \sin. \mu \times \sin. \alpha \times \cot. \delta \times \text{tang. } \frac{1}{2}\beta + \cot. \mu \times vf. \zeta$ .

EXAMPLE.

Let	AB = 94° 36' 58	
	BC = 23 28 54	
	B = 24 54 24	Cotang. 0.3331770
Diff. AB and BC =	71 8 34	Sine 9.9760412
		Cofecant AB 0.0014080
	$\mu = 26 3.44$	Cot. 10.3166262
Sin. $\mu$	9.286	
Sin. BB	9.600	
Cot. AB	8.909	
Tang. $\frac{1}{2}B$	9.344	
$\zeta = 4' 44''$ sine 7.139, this subtracted from $\mu$ leaves the angle $c = 25^\circ 59' 0''$		

This

This problem will be of use to find the right ascension of a star whose longitude and latitude, and obliquity of the ecliptic are known, or to find the Sun's azimuth at any hour in a given latitude.

I have added no cautions when these approximations and corrections change their signs, because any mathematician will discover them at sight.

I have the honour to be, &c.



XLVII. *Further Experiments and Observations in an heated Room.* By Charles Blagden, M. D. F. R. S.

Redde, July 6,  
1775.

ON the third of April, nearly the same party as before<sup>(a)</sup>, together with Lord SEAFORTH, Sir GEORGE HOME, Mr. DUNDAS, and Dr. NOOTH, went to the heated room in which the experiments of the 23d of January were made. Dr. FORDYCE had ordered the fire to be lighted the preceding day, and kept up all night; so that every thing contained in the room, and the walls themselves, being already well warmed, we were able to push the heat to a much higher degree than before. In the course of the day several different sets of experiments were going on together; but to avoid confusion, it will be necessary to relate each series by itself, without regard to the order of time; beginning with that series which serves as a continuation of our former experiments. Soon after our arrival, a thermometer in the room rose above the boiling point; this heat we all bore perfectly well, and without any sensible alteration in the temperature of our bodies. Many repeated trials, in successively higher degrees of heat, gave still more remarkable proofs of our resisting power. The last of these experiments

(a) See the former experiments, p. 111, of this volume.

riments was made about eight o'clock in the evening, when the heat was at the greatest: a very large thermometer, placed at a distance from the door of the room, but nearer to the wall than to the cockle, and defended from the immediate action of the cockle by a piece of paper hung before it, rose one or two degrees above  $260^{\circ}$ : another thermometer, which had been suspended very near the door, stood some degrees above  $240^{\circ}$ . At this time I went into the room, with the addition, to my common cloaths, of a pair of thick worsted stockings drawn over my shoes, and reaching some way above my knees; I also put on a pair of gloves, and held a cloth constantly between my face and the cockle: all these precautions were necessary to guard against the scorching of the red-hot iron. I remained eight minutes in this situation, frequently walking about to all the different parts of the room, but standing still most of the time in the coolest spot, near the lowest thermometer. The air felt very hot, but still by no means to such a degree as to give pain: on the contrary, I had no doubt of being able to support a much greater heat; and all the gentlemen present, who went into the room, were of the same opinion. I sweated, but not very profusely. For seven minutes my breathing continued perfectly good; but after that time I began to feel an oppression in my lungs, attended with a sense of anxiety; which gradually increasing for the space of a minute, I thought it most prudent to put an end to the experiment, and immediately left the room. My pulse, counted as soon as I came into the cool air, for the uneasy feeling rendered me incapable of examining

examining it in the room, was found to beat at the rate of 144 pulsations in a minute, which is more than double its ordinary quickness. To this circumstance the oppression on my breath must be partly imputed, the blood being forced into my lungs quicker than it could pass through them; and hence it may very reasonably be conjectured, that should an heat of this kind ever be pushed so far as to prove fatal, it will be found to have killed by an accumulation of blood in the lungs, or some other immediate effect of an accelerated circulation<sup>(b)</sup>; for all the experiments shew, that heating the air does not make it unfit for respiration, communicating to it no noxious quality except a power of irritating. In the course of this experiment, and others of the same kind by several of the gentlemen present, some circumstances occurred to us which had not been remarked before. The heat, as might have been expected, felt most intense when we were in motion; and, on the same principle, a blast of the heated air from a pair of bellows was scarcely to be born; the sensation in both these cases exactly resembled that felt in our nostrils on inspiration. The reason is obvious; when the same air remained for any time in contact with our bodies, part of its heat was destroyed, and consequently, we came to be surrounded with a cooler medium than the common air of the room; whereas when

(b) Since this experiment, I have observed the *mucus* from my lungs to be more *serous* than before, and to incline more to a saltish taste, though the lungs themselves seem perfectly sound in all other respects; which raises a suspicion that some of the smaller arteries suffered a degree of dilatation from the increased impulse of the blood.

fresh

fresh portions of the air were applied to our bodies in such a quick succession, that no part of it could remain in contact a sufficient time to be cooled, we necessarily felt the full heat communicated by the stove. It was observed that our breath did not feel cool to the fingers unless they were held very near the mouth; at a distance the cooling power of the breath did not sufficiently compensate the effect of putting the air in motion, especially when we breathed with force.

A chief object of this day's experiments was to ascertain the real effect of our cloaths in enabling us to bear such high degrees of heat. With this view I took off my coat, waistcoat, and shirt, and in that situation went into the room, as soon as the thermometer had risen above the boiling point, with the precaution of holding a piece of cloth constantly between my body and the cockle, as the scorching was otherwise intolerable. The first impression of the heated air on my naked body was much more disagreeable than I had ever felt it through my cloaths; but in five or six minutes a profuse sweat broke out, which gave me instant relief, and took off all the extraordinary uneasiness: at the end of twelve minutes, when the thermometer had risen almost to  $220^{\circ}$ , I left the room, very much fatigued, but no otherwise disordered; my pulse made 136 beats in a minute. On this occasion I felt nothing of that oppression on my breath which became so material a symptom in the experiment with my cloaths when the thermometer had risen to  $260^{\circ}$ : this may be partly explained by the less quickness of my pulse, the difference being at least

Vol. LXV. T t t eight

eight beats in a minute, and probably more, as in the experiment without my shirt the pulsations were counted before I had left the room; but there is a further circumstance to be taken into consideration, that the experiment attended with oppression on the breath was made in the evening after a very plentiful meal, whereas the other was made in the forenoon, some hours after a moderate breakfast. The unusual degree of fatigue which I felt from the experiment without my shirt, must be ascribed in great measure to the more violent effort which the living powers were obliged to exert, in order to preserve the due human temperature, when such hot air came into immediate contact with my body. In the present case it appears beyond all doubt, that the living powers were very much assisted by the perspiration, that cooling evaporation which is a further provision of nature for enabling animals to support great heats. Had we been provided with a proper balance, it would undoubtedly have rendered the experiment more complete to have taken the exact weight of my body at going into, and coming out of, the room; as from the quantity lost some estimate might be formed of the share which the perspiration had in keeping the body cool; probably its effect was very considerable, but by no means sufficient to account for the whole of the cooling, and certainly not equable enough to keep the temperature of the body to such an exact pitch: For it should here be remarked, that during all the experiments made this day, whenever I tried the heat of my body, the thermometer always came

very nearly to the same point; I could not perceive even the small difference of one degree, which was observed in our former experiments. Should these considerations, however, be thought insufficient to prove that evaporation was not the sole agent in keeping the body cool, I believe that Dr. FORDYCE'S experiments in moist air will be found to remove all doubts on this subject. Several of the gentlemen present, as well as myself, went into the room without shirts many times afterwards, when the thermometer had risen much higher, almost to  $260^{\circ}$ , and found that we could bear the heat very well, though the first sensation was always more disagreeable than with our cloaths.

In all the experiments made this day it was observed, that the thermometer did not sink so much in consequence of our stay in the room as on the 23d of January; probably because a much larger mass of matter had been heated by the longer continuance of the fire.

Our own observations, together with those of M. TILLET in the Memoirs of the Academy of Sciences<sup>(b)</sup>, had given us good reason to suspect, that there must have been some fallacy in the experiment with a dog, made at the desire of Dr. BOERHAAVE, and related in his Elements of Chemistry<sup>(c)</sup>. To determine this matter more exactly, we subjected a bitch weighing thirty-two pounds, to the following experiment. When the thermometer had risen to  $220^{\circ}$ , the animal was shut up in the heated room, inclosed in a basket, that its feet might be defended from

(b) For the year 1764, p. 186, &c.

(c) Tom. I. p. 275.

the scorching of the floor, and with a piece of paper before its head and breast to intercept the direct heat of the cockle. In about ten minutes it began to pant and hold out its tongue, which symptoms continued till the end of the experiment, without ever becoming more violent than they are usually observed in dogs after exercise in hot weather; and the animal was so little affected during the whole time, as to shew signs of pleasure whenever we approached the basket. After the experiment had continued half an hour, when the thermometer had risen to  $236^{\circ}$ , we opened the basket, and found the bottom of it very wet with *saliva*, but could perceive no particular *sætor*. We then applied a thermometer between the thigh and flank of the animal; in about a minute the quicksilver sunk down to  $110^{\circ}$ : but the real heat of the body was certainly less than this, for we could neither keep the ball of the thermometer a sufficient time in proper contact, nor prevent the hair, which felt sensibly hotter than the bare skin, from touching every part of the instrument. I have since found, that the thermometer held in the same place, when the animal is perfectly cool and at rest, will not rise above  $101^{\circ}$ . At the end of thirty-two minutes the bitch was permitted to go out of the room; upon coming into the cold air she appeared perfectly brisk and lively, not in the least injured by the heat, and has now continued very well above a month. Our experiment, therefore, differs, in every essential circumstance of the event, from that related by Dr. BOERHAAVE. With respect to this last it is remarkable, if the facts be properly

perly represented, that an intolerable stench arose from the dog; and that an assistant dropped down senseless upon going into the stove.

To prove that there was no fallacy in the degree of heat shewn by the thermometer, but that the air which we breathed was capable of producing all the well-known effects of such an heat on inanimate matter, we put some eggs and a beef-steak upon a tin frame, placed near the standard thermometer, and farther distant from the cockle than from the wall of the room. In about twenty minutes the eggs were taken out, roasted quite hard; and in forty-seven minutes the steak was not only dressed, but almost dry. Another beef-steak was rather overdone in thirty-three minutes. In the evening, when the heat was still greater, we laid a third beef-steak in the same place: and as it had now been observed, that the effect of the heated air was much increased by putting it in motion, we blew upon the steak with a pair of bellows, which produced a visible change on its surface, and seemed to hasten the dressing; the greatest part of it was found pretty well done in thirteen minutes.

About the middle of the day two similar earthen vessels; one containing pure water, and the other an equal quantity of the same water with a bit of wax, were put upon a piece of wood in the heated room. In one hour and an half the pure water was heated to  $140^{\circ}$  of the thermometer, whilst that with the wax had acquired an heat of  $152^{\circ}$ , part of the wax having melted and formed a film on the surface of the water, which prevented the  
evaporation:



evaporation. The pure water never came near the boiling point, but continued stationary above an hour at a much lower degree; a small quantity of oil was then dropped into it, as had before been done to that with the wax; in consequence of which, the water in both the vessels came at length to boil very briskly. A saturated solution of salt in water put into the room, was found to heat more quickly, and to an higher degree, than pure water, probably because it evaporated less; but it could not be brought to boil till oil was added, by means of which it came toward evening into brisk ebullition, and consequently had acquired an heat of  $230^{\circ}$ . Some rectified spirit of wine in a bottle slightly corked, which had been immersed into this solution of salt whilst cold, began to boil in about two hours, and soon afterwards was totally evaporated. Perhaps no experiments hitherto made furnish more remarkable instances of the cooling effect of evaporation than these last facts; a power which appears to be much greater than hath commonly been suspected. The evaporation itself, however, was more considerable in our experiments than it can be in almost any other situation, because the air applied to the evaporating surface was uncommonly hot, and at the same time not more charged with moisture than in its ordinary state. A powerful assistant evaporation must undoubtedly prove, in keeping the living body properly cool, when exposed to great heats; but it can act only in a *gross* way, and by no means in such a nice proportion to the momentary exigencies of the animal as would be requisite for the exact preservation of its temperature: that other provision of  
nature

nature which seems more immediately connected with the powers of life, is, probably, the great agent in preserving the just balance of temperature; exerting a greater effort in proportion as the evaporation is deficient, and a less effort as the evaporation increases. This idea corresponds with the general analogy of the animal economy, the nicer balances of which are almost universally effected in that part of the body which is formed with the most subtil organization.

The heated room will, I hope, in time become a very useful instrument in the hands of the physician. Hitherto the necessary experiments have not been made to direct its application with a sufficient degree of certainty. However, we can already perceive a foundation for some distinctions in the use of this uncommon remedy. Should the object in view be to produce a profuse perspiration, a dry heat acting on the naked body would most effectually answer that purpose. The histories of dropries and some other diseases, supposed to have been cured by such means, are well known to every physician. In some cases also, a moist heat, and in others heat transmitted through a quantity of cloaths, might have their peculiar advantages. That the danger likely to ensue from such applications is less than has been commonly apprehended, our former experiments gave sufficient reason to believe, and the same was amply confirmed by those which make the subject of this paper. For during the whole day, we passed out of the heated room, after every experiment, immediately into the cold air, with-

out

out any precaution; after exposing our naked bodies to the heat, and sweating most violently, we instantly went out into a cold room, and staid there even some minutes before we began to dress; yet no one received the least injury. I felt nothing this day of the noise and giddiness in my head, which had affected me in making the former experiments; and, whether from the force of habit, or any other cause, the shaking of our hands was less, and we felt less languor, though the heat had been so much more intense.

XLVIII. *A Proposal for measuring the Attraction of some Hill in this Kingdom by Astronomical Observations. By the Rev. Nevil Maskelyne, B. D. F. R. S. and Astronomer Royal.*

Reddle in the year 1772. **I**F the attraction of gravity be exerted, as Sir ISAAC NEWTON supposes, not only between the large bodies of the universe, but between the minutest particles of which these bodies are composed, or into which the mind can imagine them to be divided, acting universally according to that law, by which the force which carries on the celestial motions is regulated; namely, that the accelerative force of each particle of matter towards every other particle decreases as the squares of the distances increase, it will necessarily follow, that every hill must, by its attraction, alter the direction of gravitation in heavy bodies in its neighbourhood from what it would have been from the attraction of the earth alone, considered as bounded by a smooth and even surface. For, as the tendency of heavy bodies downwards perpendicular to the earth's surface is owing to the combined attraction of all the parts of the earth upon it, so a neighbouring mountain ought, though in a far less degree, to attract the heavy body towards its centre of attraction, which cannot be placed far from the middle of the mountain. Hence the plumb-line of a quadrant, or any other astronomical instrument, must be deflected from its proper situation by a small quantity

VOL. LXV.                      U u u                      stars,

towards the mountain; and the apparent altitudes of the stars, taken with the instrument, will be altered accordingly.

It will easily be acknowledged, that to find a sensible attraction of any hill from undoubted experiment would be a matter of no small curiosity, would greatly illustrate the general theory of gravity, and would make the universal gravitation of matter palpable, if I may so express myself, to every person, and fit to convince those who will yield their assent to nothing but downright experiment. Nor would its uses end here; as it would serve to give us a better idea of the total mass of the earth, and the proportional density of the matter near the surface compared with the mean density of the whole earth. The result of such an uncommon experiment, which I should hope would prove successful, would doubtless do honour to the nation where it was made, and the society which executed it.

Sir ISAAC NEWTON gives us the first hint of such an attempt, in his popular Treatise of the System of the World, where he remarks, "That a mountain of an hemispherical figure, three miles high and six broad, will not, by its attraction, draw the plumb-line two minutes out of the perpendicular." It will appear, by a very easy calculation, that such a mountain would attract the plumb-line  $1' 18''$  from the perpendicular.

But the first attempt of this kind was made by the French Academicians, who measured three degrees of the meridian near Quito in Peru, and who endeavoured to find the effect of the attraction of Chimborazo, a  
 5 mountain

mountain in that neighbourhood, which is elevated near four miles above the sea, though only about two miles above the general level of the province of Quito. By their observations of the altitudes of fixed stars taken with a quadrant of  $2\frac{1}{2}$  feet radius, they found the quantity of  $8''$  in favour of the attraction of the mountain, by a mean of their observations. This, indeed, was much less than they expected; but then it is to be considered, that their instrument was too small and imperfect for the purpose; and that they themselves were subject to great inconveniencies, being sheltered from the wind and weather by nothing but a common tent, and placed so high up the mountain as the boundary where the snow begins to lie unmelted all the year round. And indeed their observations, doubtless owing to these causes of error, differ greatly from one another, and are therefore insufficient to prove the reality of an attraction of the mountain Chimborazo, although the general result from them is in favour of it. Accordingly, one of the French gentlemen themselves, M. BOUGUER, who drew up the account of their experiment, expresses his wishes, that a like experiment might be made, to find the attraction of a mountain in France or England, where he thinks some might be found of sufficient bulk for the purpose. This experiment and these remarks were made in the year 1738, or above thirty years ago, yet I believe no similar experiment has ever been made in Europe.

I have made inquiries after a proper hill in this kingdom, for the trying of such a curious experiment, and

U u u 2 have

have been informed of two places in particular, extremely convenient for the purpose. The one is situated on the confines of Yorkshire and Lancashire; where, within the compass of twenty miles, are situated four very remarkable hills, called Pendle-hill, Pennygant, Ingleborough, and Whernside, which have been estimated to be from 600 to 750 yards elevated above the plane of the vales between them. By calculation on these *data*, it should follow, that the sum of the contrary attractions of Whernside, the largest of these hills, on the plumb-line placed half-way up the hill, would not be less than 30", and might amount to 46", which it is evident is a very considerable quantity, and sufficient to give us room to hope for a favourable and satisfactory success of the experiment. The other place pointed out for this purpose, is a valley two miles broad, between the hills Helwellin and Skidda, in Cumberland; which hills, according to a plan of them and the adjacent country, communicated by Mr. SMEATON, F.R.S. are elevated above 1000 yards above the intermediate valley. By a calculation made according to this plan, the sum of the contrary attractions of the plumb-line, placed alternately on the North-side of Helwellin and the South-side of Skidda, amounts to about 20", which is likewise a quantity large enough for the experiment. And although the density of the earth near the surface should be five times less than the mean density, as there is some reason to suspect, and the attractions, as here stated, should consequently be diminished in the proportion of five to one, still the sum of the contrary attractions of Whernside

side would be 6" or 9", and the sum of the contrary attractions of Helwellin and Skidda would be 4"; which quantities are not too small to be measured and demonstrated by an accurate zenith sector, such as that belonging to the Royal Society, which I made use of at St. Helena, would be, if the fault in the suspension of the plumb-line, which I there discovered, was corrected, in the manner suggested in the Philosophical Transactions, vol. LIV. p. 35 r.



XLIX. *An Account of Observations made on the Mountain Schehallien for finding its Attraction. By the Rev. Nevil Maskelyne, B. D. F. R. S. and Astronomer Royal.*

Redde, July 6, 1775. **I**N the year 1772, I presented the foregoing proposal, for measuring the attraction of some hill in this kingdom by astronomical observations, to the Royal Society; who, ever inclined to promote useful observations which may enlarge our views of nature, honoured it with their approbation. A committee was in consequence appointed, of which number I was one, to consider of a proper hill whereon to try the experiment, and to prepare every thing necessary for carrying the design into execution. The Society was already provided with a ten-feet zenith sector made by Mr. Sisson, furnished with an achromatic object glass, the principal instrument requisite for this experiment, the same which I took with me to St. Helena in the year 1761; which wanted nothing to make it an excellent instrument but to have the plumb-line made adjustable, so as to pass before and bisect a fine point at the centre of the instrument. This was ordered to be done, and a new wooden stand provided for it, capable of procuring a motion of the sector about a vertical axis, by means of which it could be more easily brought into the plane of the meridian,

meridian, or turned half round for repeating the observations with the plane of the instrument placed the contrary way, in order to find the error of the line of collimation. A large parallelopiped tent,  $15\frac{1}{2}$  feet square and 17 feet high, was also provided for sheltering both the instrument and the observer who should use it, composed of joices of wood well framed together, and covered with painted canvas. The Society was likewise possessed of most of the other instruments requisite for this experiment; as an astronomical quadrant and transit instrument made by Mr. BIRD, and an astronomical clock by SHELTON, which had all been provided on occasion of the observations of the transit of Venus in 1761 or 1769. A theodolite of the best sort was wanting, a necessary instrument for obtaining the figure and dimensions of the hill. One of Mr. RAMSDEN's construction of 9 inches diameter, was thought the fittest for the purpose, on account of the excellence of the plan on which it was made, and the number of its adjustments, being capable of measuring angles for the most part to the exactness of a single minute. The other instruments prepared for this business were, two barometers of M. DE LUC's construction, made by Mr. NAIRNE; a common Gunter's chain; a roll of painted tape three poles long, having feet and inches marked upon it; two fir poles of 20 feet each, and four wooden stands, for supporting them when used in measuring the bases, and a brass standard of five feet for adjusting them. The poles and stands were provided on the spot.

Although

Although accounts had been received from various persons of several hills supposed proper for the intended purpose, some better and some worse authenticated; yet, in order to be sure of finding the best hill for the experiment, it was determined to send a person furnished with proper instruments, to make such observations on various hills in England and Scotland, as might enable us to choose the fittest for the purpose. Accordingly Mr. CHARLES MASON, who had been employed on several astronomical occasions by the Royal Society, was appointed to make a tour through the Highlands of Scotland in the summer of the year 1773, taking notice of the principal hills in England which lay in his route either in his going or in his return. It appeared from his observations, that scarce any hill was so well adapted to the purpose as our sanguine hopes had led us to expect; for either they were not high enough, or not sufficiently detached from other hills, or their greatest length fell in a wrong direction, too near the meridian, instead of lying nearly East and West, which is a circumstance requisite to make a hill of a given height afford the greatest effect of attraction. In particular, the hills on the confines of Yorkshire and Lancashire, mentioned in the foregoing proposal, were found not to answer the description that had been given of them. Fortunately, however, Perthshire afforded us a remarkable hill, nearly in the centre of Scotland, of sufficient height, tolerably detached from other hills, and considerably larger from East to West than from North to South, called by the people of the low

low country Maiden-pap, but by the neighbouring inhabitants, Schehallien; which, I have since been informed, signifies in the Erse language, Constant Storm: a name well adapted to the appearance which it so frequently exhibits to those who live near it, by the clouds and mists which usually crown its summit. It had, moreover, the advantage, by its steepness, of having but a small base from North to South; which circumstance, at the same time that it increases the effect of attraction, brings the two stations on the North and South sides of the hill, at which the sum of the two contrary attractions is to be found by the experiment, nearer together; so that the necessary allowance of the number of seconds, for the difference of latitude due to the measured horizontal distance of the two stations in the direction of the meridian, would be very small, and consequently not subject to sensible error from any probable uncertainty of the length of a degree of latitude in this parallel. For these reasons the mountain Schehallien was chosen, in preference to all others, for the scene of the intended operations; and it was concluded to make the experiment in the summer of the year 1774.

It was foreseen, that this experiment would be attended with considerable expence, and such as might easily have exceeded the common funds of the Royal Society, without some extraordinary assistance. The bounty of his Majesty, our Patron, happily removed this difficulty. At the humble request of the Society, his Majesty had been graciously pleased to grant a very ample

sum to their disposal, for defraying the expences of the observations of the late transit of Venus in 1769, as his Majesty had before done with respect to the former transit of Venus in 1761. Out of this benefaction, after all expences had been paid, there was a considerable remainder, and, the Society humbly requesting to know his Majesty's pleasure about the disposal of it, he was graciously pleased to direct them, to *lay it out in such manner as they thought proper, and was most agreeable to the end of their institution.* As this bounty of his Majesty had been originally granted for an astronomical purpose, the Society thought they could not dispose of it on any more important object, or in any manner more consistent with the intentions of their Royal Patron and Benefactor, than by expending it on this astronomical experiment of the attraction of a mountain, as what could hardly fail of throwing light on the principle of universal gravitation, and was likely to lead to new discoveries concerning the constitution of this earth which we inhabit, particularly with respect to the density of its internal parts.

The experiment being thus resolved upon, the next next thing to be done was to fix on a proper person to carry it into execution. Numerous and interesting as my literary engagements are at the Royal Observatory, I had no thoughts of undertaking this care and labour myself, till the Council of the Royal Society were pleased to do me the honour to think my assistance necessary to insure the success of so important and delicate an experiment. Their thinking so was a sufficient motive with me to

encounter whatever difficulties and fatigues might attend operations carried on in so inconvenient and inclement a situation. But it was requisite I should also have his Majesty's permission for absenting myself so long from my duty at the Royal Observatory. This his Majesty was graciously pleased to grant; and to allow me to stay as long as I thought necessary, to complete *my very important observations*.

Such were the motives for undertaking this experiment, and the preparations made for putting it in execution. I am now to give an account of the operations themselves.

The quantity of attraction of the hill, the grand point to be determined, is measured by the deviation of the plumb-line from the perpendicular, occasioned by the attraction of the hill, or by the angle contained between the actual perpendicular and that which would have obtained if the hill had been away. The meridian zenith distances of fixed stars, near the zenith, taken with a zenith sector, being of all observations hitherto devised capable of the greatest accuracy, ought by all means to be made use of on this occasion: and it is evident, that the zenith instrument should be placed directly to the North or South of the centre of the hill, or nearly so. In observations taken in this manner, the zenith distances of the stars, or the apparent latitude of the station, will be found as they are affected by the attraction of the hill. If then we could by any means know what the zenith distances of the same stars, or what the latitude of the place would have been, if the hill had  
X x x 2
been

been away, we should be able to decide upon the effect of attraction. This will be found, by repeating the observations of the stars at the East or West end of the hill, where the attraction of the hill, acting in the direction of the prime vertical, hath no effect on the plumb-line in the direction of the meridian, nor consequently on the apparent zenith distances of the stars; the differences of the zenith distances of the stars taken on the North or South side of the hill, and those observed at the East or West end of it, after allowing for the difference of latitude answering to the distance of the parallels of latitude passing through the two stations, will shew the quantity of the attraction at the North or South station. But the experiment may be made to more advantage on a hill like Schehallion, which is steep both on the North and South sides, by making the two observations of the stars on both sides; for the plumb-line being attracted contrary ways at the two stations, the apparent zenith distances of stars will be affected contrary ways; those which were increased at the one station being diminished at the other, and consequently their difference will be affected by the sum of the two contrary attractions of the hill. On the South side of the hill, the plumb-line being carried Northward at its lower extremity, will occasion the apparent zenith, which is in the direction of the plumb-line continued backwards, to be carried Southward, and consequently to approach the equator; and therefore, the latitude of the place will appear too small by the quantity of the attraction; the distance of the equator from  
the

the zenith being equal to the latitude of the place. The contrary happens on the North side of the hill; the lower extremity of the plumb-line being there carried Southward will occasion the apparent zenith to be carried Northward or from the equator; and the latitude of the place will appear too great by the quantity of the attraction. Thus the lesser latitude appearing too small by the attraction on the South side, and the greater latitude appearing too great by the attraction on the North side, the difference of the latitudes will appear too great by the sum of the two contrary attractions; if therefore there is an attraction of the hill, the difference of latitude by the celestial observations ought to come out greater than what answers to the distance of the two stations measured trigonometrically according to the length of a degree of latitude in that parallel, and the observed difference of latitude subtracted from the difference of latitude inferred from the terrestrial operations, will give the sum of the two contrary attractions of the hill. To ascertain the distance between the parallels of latitude passing through the two stations on contrary sides of the hill, a base must be measured in some level spot near the hill, and connected with the two stations by a chain of triangles, the direction of whose sides, with respect to the meridian, should be settled by astronomical observations.

If it be required, as it ought to be, not only to know the attraction of the hill, but also from thence the proportion of the density of the matter of the hill to the mean density of the earth; then a survey must be made  
of



of the hill to ascertain its dimensions and figure, from whence a calculation may be made, how much the hill ought to attract, if its density was equal to the mean density of the earth; it is evident, that the proportion of the actual attraction of the hill to that computed in this manner will be the proportion of the density of the hill to the mean density of the earth.

Thus there were three principal operations requisite to be formed. 1. To find by celestial observations the apparent difference of latitude between the two stations chosen on the North and South sides of the hill. 2. To find the distance between the parallels of latitude. 3. To determine the figure and dimensions of the hill.

I arrived at the hill of Schehallien on the last day of June, and found the observatory and instruments there, which had been brought down some time before from London to Perth on board a ship, and thence conveyed over land to the hill under the care of Mr. REUBEN BURROW, my late assistant at the Royal Observatory. The observatory was fixed half-way up the South side of the hill, as the place where the effect of the hill's attraction would be at the greatest, and it was placed in the like manner when it was afterwards removed to the North side. A circular wall was raised, five feet in diameter, and covered at top with a moveable conical roof for sheltering the astronomical quadrant; and a square tent was put up for receiving the transit-instrument, all near to the observatory. A *boobie*, or temporary hut, was also made near it, for my residence, while I was attending

the astronomical observations on this side of the hill. I first put the sector, nearly in the meridian, by means of the variation compass; but, thro' the badness of the weather, which was almost continually cloudy or misty, I could not before the middle of July get a sufficient number of observations with the astronomical quadrant, to know the state of the clock, in order to draw a meridian line on the floor of the observatory, for setting the sector truly in the plane of the meridian. The first observations, which I made with the sector, after it was put truly in the meridian, were on the 20th of July. Between this time and the end of the month, I observed the zenith distances of 34 stars, some to the North and some to the South of the zenith; and many of them several times over, having taken 76 observations in all, with the plane of the sector turned to the East. On the first of August, I turned the plane of the instrument about, to face the West, and set it in the meridian again, by means of the meridian line drawn on the floor the 26th of July, and secured by picquets driven into the ground; and between that and the 15th of the same month, I observed 39 stars including most of those taken in the former position of the instrument, and took 93 observations in all.

And here let me take notice of a method which I fell upon of verifying the position of the sector, with respect to the plane of the meridian, which, had I thought of it at first, would have saved me much trouble; and therefore I will now mention it, as it may be useful to future observers. It consists in observing the transits of two stars,

stars, differing considerably in declination from one another across the vertical wire of the sector, and comparing the observed difference of their transits with the known difference of their right ascensions. If they agree, it may be safely concluded, that the instrument is truly placed in the meridian. If not, by comparing the alteration that would be produced in the difference of the transits, by supposing the instrument out of the meridian, by any small quantity, as one degree or ten minutes, with the observed error, the deviation of the instrument from the meridian may be inferred. In this manner I found, that the instrument had been put very exactly in the meridian by means of the meridian line; the difference by the two methods coming out only  $2\frac{1}{2}$  minutes of azimuth. As to the continuance of the instrument in the plane of the meridian, I had a constant proof of it by the same means, and likewise a farther security, which I did not fail to attend to, by noting the degree and minute which an index depending on the vertical axis of the instrument pointed out on a fixed azimuth circle. Being apprehensive of error in an instrument supported on a wooden frame, I frequently examined the parallelism of the fore arch to the back arch, by measuring their perpendicular distances at the two ends with a brass scale, whose vernier shewed the five hundredth part of an inch, and found it liable to variations of a minute or two, owing probably to the force used in setting the sector to different zenith distances, and the weakness of some screws at the top of the frame; which small error I corrected, till I found it liable

to continual returns: and I satisfied myself, that the plane of the sector never deviated above three minutes from the meridian in any of the observations taken on the South side of the hill, which, it is evident, could not in the least affect the observed zenith distances of stars. I hardly ever observed without examining the bisection of the point at the centre of the instrument, by the plumb-line; which was absolutely necessary, on account of the gradual changes of the wooden frame. My view in mentioning these minute circumstances is, to caution future observers, as well as to confirm my own observations. But whoever makes use of an instrument of this kind, supported on a wooden frame, will find the greatest attention necessary to attain the same degree of accuracy in his observations, as if his instrument were fixed to an immoveable wall. In the mean time, by observations taken with the quadrant and transit instrument, I got a meridian line, and planted a pole to preserve it on the top of the hill, to the South of the instrument, and another at the foot of the same hill; from whence, by measuring off an equal distance to the East (as the South-west corner of the observatory lay to the East of the transit instrument) and setting up another pole, another meridian line was got, passing through the South-west corner of the Southern station of the observatory. The reason for making the meridian line pass through the South-west corner of the observatory rather than through the middle of it was, that this part of it had been taken when

the observatory had been used as an object in taking angles by the theodolite, in the survey of the hill.

While I was engaged in these astronomical observations, Mr. BURROW, attended by Mr. WILLIAM MENZIES, a land-surveyor living in the neighbourhood, who had been recommended by some of the principal gentlemen of the country, as a proper person for this work, went out every day that the weather permitted, to take sections of the hill, and angles between several objects, for determining the figure and dimensions of the hill. The method made use of was this, which was proposed by Mr. BURROW, and was well adapted to the purpose. A number of station poles were set up at convenient distances all round the foot of Schehallien; but rather without its base, and chiefly on little eminences rising from the foot of it, which formed a polygon of many sides, surrounding the hill; and when delineated on paper, shew very nearly the shape of its base. At each station, the angular position of two or more of the other stations being observed with the theodolite, and one side being determined by means of a measured base, all the other sides will be known. From these stations, sections of the hill up to the top were taken in the following manner. The theodolite, being placed at any station, was pointed towards the hill; and a labourer was sent with a number of poles, which he was to plant in the ground truly upright, at regular distances and in a vertical plane, according to signals which he received from the person that stood at the theodolite, who also took the altitude of the foot of  
each

each pole, and likewise the horizontal angle contained between the plane of the section poles and the next station pole to the right or left. The theodolite was then removed, and planted directly over the centre of this station pole, which was removed for this purpose; and the horizontal angle taken between a pole now planted at the first station and each of the poles of the section. The horizontal distance of the two station poles being known, the horizontal distance of each of the section poles from the first station, and their respective perpendicular altitudes above it, or depth below it, will be given.

It is manifest, that these operations, when connected by angles with the two stations of the observatory and the meridian line, would at the same time give the shape and dimensions of the hill, and the distance of the parallels of latitude passing through the two stations of the observatory, as well as their respective elevations above the base of the hill. But errors being apt to accumulate in a long chain of triangles; to obviate this danger, as well as to produce a check on any great mistakes, that might happen to be made in reading off, or writing down, the angles, I caused a heap of stones, or *carn* as it is called by the people of the country, to be raised in a circular figure six feet high, at the highest point of the ridge of the hill, which is to the West of it, as a signal to be observed from the several angles of the polygon, and as a means of connecting the two stations of the observatory by a smaller number of triangles. Another *carn* towards the Eastern end of the ridge of the hill was

afterwards set up for the like purpose. I proposed to determine the distance of the two *carns* by connecting them by angles with a base, to be measured in a level spot in the vale below the hill, and then to make use of the said distance as a secondary base for determining the sides of the polygon, and the distance of the two stations of the observatory. Had the two *carns* been visible from the two stations of the observatory, two triangles would have sufficed for connecting the two stations together. But, notwithstanding that this was not the case, and that only the two *carns* were visible from one another, yet all the angles of these two triangles were measured by Mr. BURROW in the following method, suggested by himself. He went with the theodolite to the neighbouring hill on the South side of Schehallien, which runs parallel to it; and, by varying his situation, found a point whence the Western *carn* and Southern observatory appeared by the theodolite to be in one vertical plane, and removing the theodolite he planted a pole there. In like manner he planted another pole on the same hill, in a vertical plane with the Southern observatory and Eastern *carn*. Then returning to the observatory, he took the horizontal angle contained between the two poles, which it is evident is equal to its opposite angle, or that contained between the *carns*. And going to the West *carn*, he took the angle contained between the East *carn* and the pole planted on the opposite hill, in a line with the Southern observatory and West *carn*, which is the same with the angle between the East *carn* and Southern Observatory. And lastly, going to the East *carn*, he took the angle contained

tained between the Western *carn* and the pole placed on the opposite hill in a line with the East *carn* and Southern observatory, which is the same with the angle contained between the Western *carn* and Southern observatory. Thus were the three angles found of the triangle made by the Southern observatory and two *carns*. In the like manner were the angles of the triangle made by the Northern observatory and two *carns* found afterwards. And, as a proof that the angles of the two triangles were rightly determined, their sum in the first case differed from  $180^{\circ}$  by little more than two minutes; and in the second case by only half a minute.

Notwithstanding the advantages which attended this method of finding the distance of the two stations of the observatory, I thought it proper to make use also of the other method of doing the same thing by a small number of triangles carried directly across the hill, thinking it expedient, in a matter of such consequence, to rely on no single operation; but, as far as possible, to confirm every deduction by another found in an independent manner. I had caused two poles to be set as far up the hill of Schehallien as they could be placed; one as near the Western, and the other the Eastern *carn* as they could be, so as to be visible from the Southern station of the observatory: also two others in like manner visible from the North observatory; one of which was very near the East *carn*, and the other only 269 feet distant from the Westernmost of the two poles visible from the South observatory; so narrow was the ridge of the hill in that part, although it grew wider both to the West and East,



East, but much more towards the latter. With these four poles, the East *carn*, and the two stations of the observatory, five triangles were formed, connecting the two stations of the observatory, the relative situation of which to each other would be determined as soon as the length of any one of the sides of these triangles was known, either by comparing it with a base measured in the valley below, or with the distance of the two *carns* settled in that manner.

I had got sufficient observations of zenith distances of stars with the sector on the South side of the hill by the 15th of August; I prepared therefore for removing the observatory and instruments to the new station on the North side. This was a work of great labour and difficulty, as every thing was carried over the ridge of the hill on men's shoulders, and some of the packages were very weighty; it employed the labour of twelve men for a week, and was compleated on the 26th. A large level area had been cut away, with great labour, here, in the side of the hill, for receiving the observatory, as had before been done on the South side of the hill. A new *booth* was also erected, and places for holding the quadrant and transit instrument, as before, adjoining to the observatory.

The badness of the weather prevented me from beginning my observations with the sector till the 4th of September; but, that being a clear night, I had a fair opportunity of putting in practice the method of bringing the instrument into the meridian by the transits of the stars

stars across the plane of the sector, which was mentioned before. The sector being put up with its plane facing the West, and set near the meridian by the variation compass, allowing for the variation, I found, by the transit of  $\alpha$  Draconis, on the North side of the zenith, compared with those of  $\kappa$ ,  $\iota$  and  $\theta$  Cygni on the South side, that the instrument deviated  $49\frac{1}{2}$  minutes to the West of the South in azimuth; which being corrected, by turning the instrument about on its vertical axis, towards the East, by the help of the divisions on the azimuth circle; I then found, by the transit of  $\eta$  Cephei, on the North side of the zenith compared with that of  $\pi$  Cygni on the South side, that the instrument deviated seven minutes to the East of the South in azimuth, which I corrected accordingly. And so near was it brought to the meridian in this manner, that by the most exact comparison of the transits of several stars on the 7th and 8th instant, it appeared to be only two minutes out of the meridian, and that to the East of the South; which small error I also attempted to correct; but the instrument rested one minute out of the position which I intended to give it, owing to the difficulty of turning it about to such great nicety, and so I let it remain.

It was indeed a most fortunate circumstance, that I thus got the instrument so near the meridian by the very first night's observations, those of September 4th; for the badness of the weather in the day prevented me from getting a meridian line by the Sun till the 15th. Had I therefore been obliged to wait for setting the instrument right by the Sun; I should

I should have lost four good days observations, which were two-thirds of those I took on this side of the hill with the plane of the instrument turned to the West, and been retarded near three weeks in my observations; and, as the opportunities of weather fit for observing at all were but very rare, I might have been thereby thrown back into the winter, and defeated of making so complete a set of observations on the North side of the hill as I had got on the South side, whose correspondence would thereby have been rendered less perfect. I had the satisfaction, however, when I drew the meridian line on the floor of the observatory by the equal altitudes of the Sun taken on the 15th, to find it agree perfectly, even to the same minute, with the position of the instrument, as determined by the transits of the stars. But no one will doubt of the superior ease and readiness afforded by the latter method, in preference to the other.

On the 20th of September I completed the observations with the plane of the sector turned to the West, having observed 32 stars, and taken 68 observations in all. On the 22d, I turned it about with the plane to face the East, and set it again in the meridian, by putting it parallel to its former position, by means of the meridian line secured by marks made on picquets let into the ground perpendicularly below the plane of the instrument, before it was turned. Between this time and the 24th of October, I observed 37 stars, and took 100 observations in all, with the plane of the instrument facing the east:  
and

and thus I completed my whole series of observations with the sector, having observed 43 different stars in all, on both sides of the hill, and taken 337 observations.

As a few observations, taken with so excellent an instrument as this zenith sector, would have been sufficient to determine the apparent difference of latitude of the two stations of the observatory, to a second or two; I am apprehensive I may be thought by many to have multiplied observations unnecessarily. However that may be, I apprehend, that doubling the observations in each station of the observatory, by taking them with the plane of the instrument alternately facing the East and West, will be allowed to be a proper step, as the line of collimation of the instrument is hereby separately determined at each station, and thereby all danger of any alteration happening in the same, in its removal from one side of the hill to the other, is intirely obviated. I had, indeed, all the reason in the world to think, that the sector was carried from one station to the other without the least accident; but still it was proper to guard against what was possible to happen.

But I had reasons also for multiplying the observations made in the same position of the instrument. It was important to demonstrate the exactness of the instrument from the near agreement of a number of observations taken with it, as its excellence was not to be intirely presumed, unless this proof could be shewn in its favour. Besides, it might be expected, that some unsteadiness or warping

ing of the wooden stand on which it was supported, might affect the accuracy of the observations; or there might be variable and discordant refractions, even near the zenith, on the side of so steep a hill, more than are found in lower situations. Add to this, that when I began my observations on the South side of the hill, having a prospect of bad weather before me, and not knowing how few observations I might be able to get on either side of the hill, I thought it prudent to endeavour to observe most of the stars in the British catalogue, which came within the reach of the instrument, that I might be sure of being provided with observations of some at least of the same stars, which I might afterwards observe when I should be removed to the North side of the hill; where, after an interval of perhaps some months, many stars, that before passed the meridian in the night, would pass it in the day, and consequently be either invisible through the telescope of the sector, or more precarious of being seen.

Although a meridian line had been found by the transit instrument at the South observatory, whereby the relative situation of the two stations of the observatory, as well as of the other points of the hill, with respect to the meridian, might be determined; yet I judged it would be more satisfactory to confirm this by another meridian line drawn at the Northern observatory. This I found, as I had done the former, by setting the transit instrument to agree with the pole-star at the computed time of its passing the meridian, and confirmed it by comparing

I      2      3      4      5      6      7      8      9      10      11      12

the

the difference of the transits of the pole-star and of  $\alpha$  Pegasi,  $\alpha$  Andromedæ, and  $\gamma$  Pegasi, with their difference of right ascension, in the same manner by which I had put the sector in the plane of the meridian, and found it to agree with the former meridian line within two minutes.

It remains to give an account of the manner in which the two bases were measured; one in a level spot at the foot of the hill, to the Southward; and the other at the distance of about  $2\frac{1}{2}$  miles from the hill to the North-west, in the plain of Rannock. I caused two measuring poles to be made of straight-grained well-seasoned fir, in the form of square tubes, 3 inches square and 20 feet long, and strengthened with square pieces within-side at several distances. These were carefully compared with the brass standard made by Mr. BIRD, the same which was used in the measure of the degree at Pennsylvania, immediately before they were applied to the measure of the bases, and the height of the thermometer noted at the time in order to make allowance for the expansion or contraction of the brass standard by heat or cold. Four wooden stands were provided for supporting the poles; each having a triangular base with three iron spikes beneath, at each of the angles. An upright pole, six feet high, rose from the middle of one side of the triangle, and two short braces were joined to it from the two ends of this side, and a long slant pole from the opposite angle. Two sliding arms were put upon the upright pole, capable of being raised or depressed, one above and the other below the

place where the slant pole was fastened to the upright pole, for supporting the measuring poles at a convenient height above the ground. In measuring the base, one end of a pole was supported on one of the stands, and the other end on another stand; and it was set horizontal by means of a spirit level laid on it about the middle, and by raising or depressing the arm on which it rested at one or the other end. The other pole was then, in like manner, supported on the two other stands truly level, and in the same vertical plane with the former pole, namely, that of the intended base, without regarding whether they were exactly of the same height, and with some small horizontal interval between their ends. This interval was measured by laying one leg of a brass rectangle, which was divided into inches and tenths, along one pole, while the other, or vertical leg, touched the end of the other pole: for it was not thought advisable, to bring the ends of the poles to touch exactly, as that would have taken up a great deal of time, and might have endangered the altering the position of the hindermost pole, if it should chance to receive any shock by laying down the foremost pole. It is evident, that the inches and tenths given by the divisions of the brass rectangle are to be added into one sum together with the poles, in computing the length of the base. When the foremost pole was truly placed, and the interval between them had been measured by the divided side of the brass rectangle, the hindermost pole was taken up, and the stands on which it had rested were advanced forwards, and the pole again laid

on

on them, truly level, and in the true direction of the base. In order to set the poles continually in the proper direction of the base, the following method was used. The theodolite was first set up at one end of the base, and an upright pole at the other, and another in the middle, and a third was from time to time advanced to a little distance forward; and the measuring poles were sometimes placed in the proper direction by the eye, looking along the lengths of both poles together to the upright pole before them, and sometimes by the help of the theodolite. In this manner, about the middle of September, a base was measured by Mr. BURROW and Mr. MENZIES of 3012 feet, in the valley at the foot of the hill to the South-west; but not so accurately as this method is capable of, owing to the stands being very unsteady, through the looseness of the spikes in the feet and other faults, during the measuring the first quarter of the base, though they were mended before the mensuration of the remainder of it. The mensuration of another base of the length of 5897 feet, in the meadow of Rannock, about  $2\frac{1}{2}$  miles to the South-west of the centre of the hill, which I attended myself, was performed with the greatest accuracy, according to the same method, on the 10th, 11th, and 12th of October, with new stands, more substantial and firm than the former.

The extreme badness of the weather no less retarded the operations of the survey than the celestial observations; for there was almost constant rain, mist, or high wind, to obstruct the use of the theodolite: indeed



all the people of the country agreed, it was the worst season that had ever been known. So that it was not till the 20th of October that the sections had been carried all round the hill. Nor would this work have been so much forwarded as it was, had it not been for the use of an additional theodolite of the same construction, and by the same maker, as the former, which was lent me, upon my request, by the right honourable JAMES STUART MACKENZIE, lord privy seal for Scotland; who, having long cultivated a distinguished taste for astronomy, was pleased to honour the experiment of attraction with every assistance, which his interest or recommendation could procure. I am particularly to acknowledge the favour he conferred upon me by introducing me to the acquaintance of Sir ROBERT MENZIES, baronet, his brother-in-law, a gentleman conversant in mathematical and philosophical learning, who honoured me with his friendship during my residence in the country; and, besides many personal civilities shewn to myself, rendered many material assistances to the main purpose of carrying on the experiment. It is with pleasure also, that I acknowledge the civilities of all the neighbouring gentlemen, who often paid me visits on the hill, and gave me the fullest conviction that their country is with justice celebrated for its hospitality and attention to strangers. I was honoured also by visits from many learned gentlemen who came from a great distance; particularly the lord privy seal, Dr. WILSON, professor of astronomy at Glasgow, and his son,

son, and Dr. REID, professor of moral philosophy, and Mr. ANDERSON, professor of natural philosophy, also at GLASGOW, lord POLWARTH, Mr. RAMSAY, professor of natural history at Edinburgh, Mr. Commissioner MENZIES of the Customs at Edinburgh, Mr. COPLAND and Mr. PLAYFORE, of the university of Aberdeen, the rev. Mr. BRICE, and my esteemed friend Col. ROY, who had been my companion in the journey as far as Edinburgh. So great a noise had the attempt of this uncommon experiment made in the country, and so many friends did it meet with interested in the success of it!

The use of the two theodolites at once, as mentioned above, much forwarded the completing of the sections all the month of October; Mr. MENZIES observing the bearings at one station with one theodolite, while Mr. BURROW observed the altitudes or depressions with the other theodolite at the other station; and the labourer, who used to plant the poles in the hill, taking only one pole with him, and fixing it up at one place to be observed at both theodolites, and then removing it to the next station for the like purpose. Notwithstanding which, the weather grew at length so bad, by the early coming in of frost and snow in the beginning of November, when the survey was near completed, as to render it impossible to do any thing more that season. It became therefore necessary to finish this astronomical campaign, leaving the theodolite in the care of Mr. MENZIES, to complete what little remained to be done the next season.

I have

I have thus described the plan which was adopted for the operations on Schehallien, and the manner in which it was carried into execution; it only remains to give the result of computations made upon those operations for deducing the effect of the attraction of the mountain. The operations themselves at large shall be communicated another opportunity.

I had caused the arch of the sector to be divided by fine points, according to a new and arbitrary division adapted to the method of continual bisection. One-eighth part of the radius of the instrument was found by three bisections, and applied as a chord to the arch from the middle on each side, intercepting each way

$7^{\circ} 9' 59,917$ . These spaces were each divided by points into 128 parts, by continual bisection; therefore one division will contain  $3' 21,561854$ . Hence the number of degrees and minutes answering to any number of divisions may easily be found. Twenty-four additional parts were also set off taken from the former, and added at the 128th division on each side, to fill up the whole extent of the arch, which thus consisted of 152 divisions on each side of the centre, answering to an angle of

$8^{\circ} 30' 37,4$ , which was therefore the greatest angle the instrument was capable of measuring. To find the value of the parts of the micrometer in seconds, I measured the distance of the points on the limb, by five at a time, by means of the plumb-line, in parts of the micrometer from

from 0 to 128 on each side of the middle or point marked 0 upon the arch. By a mean of all these measures, one division of the arch or  $3^{\circ} 21,562''$  came out equal to four revolutions and 34,8272 parts of the micrometer, 41 of which make one revolution; and therefore one part is equal to  $1,0137545''$ , and 41 or one revolution is equal to  $41,5639345''$ . Hence the value of any number of revolutions and parts of the micrometer may be easily found. At all observations of the same star, whether on the North or South side of the hill, I brought the same point of the arch, namely, that which agreed nearest with the zenith distance of the star, under the plumb-line, so that the difference of the apparent zenith distances of the same star on contrary sides of the hill is given in parts of the micrometer, and has no reference to the divisions of the instrument, whether they be equal or unequal; and, the parts of the micrometer screw being perfectly equal, as I had formerly satisfied myself by measuring the interval of two given points on the arch with different parts of the screw, that difference of apparent zenith distances may be perfectly relied on, as far as depends upon the instrument, provided the bisection of the star by the wire in the telescope, and that of the point on the arch by the plumb-line, were accurately performed. As the plane of the instrument was placed both East and West, at both stations of the observatory, the difference of the latitude of the two stations may be found

as well from the observations made in one position of the instrument as the other. If the instrument had suffered no change by being carried over the hill, that is to say, if the line of collimation was not altered thereby, the results should come out equally true from the observations taken in both positions of the instrument. On the contrary, if the line of collimation should, by any means, have suffered any alteration between the observations made at the two stations, this would cause the difference of latitude to appear too small, by the observations made in one position of the instrument, by the quantity of the alteration, and as much too great, in the other position of the instrument. But still the mean between the two results, deduced from the observations taken in the two different positions of the instrument, would give the true difference of latitude; and that equally, whether the line of collimation had suffered any change or not. Therefore this will be the best method of comparing the observations together, and I shall take a mean of all the results, deduced from the observations taken in each position of the instrument separately, and then a mean of those means for the true difference of latitude. By single observations of ten stars; *viz.*  $\beta$ ,  $\alpha$ , and  $\eta$  Cassiopeæ, and  $\gamma$ ,  $\eta$ ,  $\beta$ , 39, 45, 46, and 53 Draconis, made on both sides of the hill, with the plane of the sector facing the West, after making the proper allowance for precession, aberration, and deviation, and semi-annual solar nutation of the earth's axis (see my tables annexed to my Observations made at the Royal Observatory), the apparent difference

ference of latitude between the two stations of the observatory, comes out  $54,1$ ,  $54,7$ ,  $54,0$ ,  $55,4$ ,  $55,0$ ,  $55,0$ ,  $52,2$ ,  $54,0$ ,  $54,3$ ,  $53,1$ , respectively; the mean of all which is  $54,2$ ; the greatest difference of any one result from the mean being only  $2''$ . In like manner, by single observations of as many stars; *viz.*  $\beta$  and  $\alpha$  Cassiopeæ;  $\epsilon$  Ursæ majoris;  $\beta$ ,  $39$ ,  $46$ ,  $0$ ,  $49$ , and  $53$ , Draconis; and  $23$  Cygni; made on both sides of the hill, with the plane of the sector facing the East; after making all the allowances as before, the apparent difference of latitude comes out  $54,5$ ,  $52,3$ ,  $56,8$ ,  $53,5$ ,  $54,5$ ,  $57,2$ ,  $56,1$ ,  $55,3$ ,  $54,1$ ,  $55,1$ , respectively; the mean of all which is  $55''$ ; the greatest difference of any one result from the mean being  $2''$ . The two means  $54,2$  and  $55,0$  differ only  $0,8$ , which should argue only an alteration of  $0,4$  in the line of collimation; but this is too small a quantity to be depended on; and therefore it is most probable, that the state of the instrument remained unvaried. However, whether it did or no, the mean of the two means, or  $54,6$ , is to be esteemed the apparent difference of latitude between the two stations of the observatory, and, when compared with the difference of latitude which should result from the trigonometrical measures, will give the sum of the two contrary attractions of the hill.

It must be owned, that this point will be settled with rather more certainty when all the observations made with the sector, which exceed 300, shall have been computed; but, as from the agreement of these results together, as well as from the small differences that are usually found in observations made within a few days of one another, one may presume, that the result from the whole will not differ materially from that deduced above from 40 observations, I thought I had better take this opportunity of gratifying the impatience of the Society in presenting them with these my first computations before their summer recess, than delay giving them any account at all of this experiment, till I had leisure to complete the whole of my calculations.

I am now to shew, what the distance is between the parallels of latitude passing through the two stations of the observatory in feet, according to the trigonometrical mensuration; and thence, what the difference of latitude ought to have been, if the hill had been away, or had exerted no sensible attraction. This depends on the enumeration of several particulars.

The length of the base measured in the meadow of Rannoch was 5897,119 feet, according to the state of the brass standard when the thermometer was at  $40^{\circ}$ ; but, to reduce it to answer to the state of the brass standard in the heat of  $62^{\circ}$ , we must subtract 16,721 feet; we should also subtract farther 0,327, for the diminution which the brass standard has suffered by wear, and there remains 5880,071 feet for the true length of the

the base in Rannoch. See Phil. Transf. vol. LVIII. p. 313. 324. and 326. Hence, with the help of the angles taken with the theodolite at the ends of the base in Rannoch, and at the West *carn*, the horizontal distance between the East and West *carns* comes out 4047,4 feet. Nearly the same result comes out from the base measured on the South side of the hill, though with less exactness; this, when all corrections are made, is 3011,684 feet, whence the distance of the two *carns* should come out 4058 feet, or about ten feet longer than results from the base in Rannoch. But I prefer the deduction from the base in Rannoch as most to be depended on. Hence, by the calculation of the two triangles formed by the two *carns* and the two stations of the observatory, the distance between the parallels of latitude passing through the two stations comes out 4364,4 feet, which, according to M. BOUGUER's table of the length of a degree in this latitude of  $56^{\circ} 40'$ , at the rate of 101,64 English feet to one second, answers to an arc of the meridian of  $42,94''$ . The other series of triangles carried across the hill, gives the same distance of the parallels only 10 feet less, and consequently the arc of the meridian only  $\frac{1}{10}$ th of a second less. Thus the difference of latitude found by the astronomical observations, comes out greater than the difference of latitude answering to the distance of the parallels, the former being  $54,6''$ , the latter only  $42,94''$ . The difference  $11,6''$  is to be attributed to the sum of the two contrary attractions of the hill.



The attraction of the hill, computed in a rough manner, on supposition of its density being equal to the mean density of the earth, and the force of attraction being inversely as the squares of the distances, comes out about double this. Whence it should follow, that the density of the hill is about half the mean density of the earth. But this point cannot be properly settled till the figure and dimensions of the hill have been calculated from the survey, and thence the attraction of the hill, found from the calculation of several separate parts of it, into which it is to be divided, which will be a work of much time and labour; the result of which, will be communicated at some future opportunity.

Having thus come to a happy end of this experiment, we may now consider several consequences flowing from it, tending to illustrate some important questions in natural philosophy.

1. It appears from this experiment, that the mountain Schellien exerts a sensible attraction; therefore, from the rules of philosophizing, we are to conclude, that every mountain, and indeed every particle of the earth, is endued with the same property, in proportion to its quantity of matter.

2. The law of the variation of this force, in the inverse *ratio* of the squares of the distances, as laid down by SIR ISAAC NEWTON, is also confirmed by this experiment. For, if the force of attraction of the hill had been only to that of the earth, as the matter in the hill to that of the earth, and had not been greatly increased by the near approach

proach to its centre, the attraction thereof must have been wholly insensible. But now, by only supposing the mean density of the earth to be double to that of the hill, which seems very probable from other considerations, the attraction of the hill will be reconciled to the general law of the variation of attraction in the inverse duplicate *ratio* of the distances, as deduced by Sir ISAAC NEWTON from the comparison of the motion of the heavenly bodies with the force of gravity at the surface of the earth; and the analogy of nature will be preserved.

3. We may now, therefore, be allowed to admit this law; and to acknowledge, that the mean density of the earth is at least double of that at the surface, and consequently, that the density of the internal parts of the earth is much greater than near the surface. Hence also, the whole quantity of matter in the earth will be at least as great again as if it had been all composed of matter of the same density with that at the surface; or will be about four or five times as great as if it were all composed of water. The idea thus afforded us, from this experiment, of the great density of the internal parts of the earth, is totally contrary to the hypothesis of some naturalists, who suppose the earth to be only a great hollow shell of matter; supporting itself from the property of an arch, with an immense vacuity in the midst of it. But, were that the case, the attraction of mountains, and even smaller inequalities in the earth's surface, would be very great, contrary to experiment, and would affect the measures

measures of the degrees of the meridian much more than we find they do; and the variation of gravity in different latitudes in going from the equator to the poles, as found by pendulums, would not be near so regular as it has been found by experiment to be.

4. The density of the superficial parts of the earth, being, however, sufficient to produce sensible deflections in the plumb-lines of astronomical instruments, will thereby cause apparent inequalities in the mensurations of degrees in the meridian; and therefore it becomes a matter of great importance to chuse those places for measuring degrees, where the irregular attractions of the elevated parts may be small, or in some measure compensate one another; or else it will be necessary to make allowance for their effects, which cannot but be a work of great difficulty, and perhaps liable to great uncertainty.

After all, it is to be wished, that other experiments of the like kind with this were made in various places, attended with different circumstances. We seldom acquire full satisfaction from a single experiment on any subject. Some may doubt, whether the density of the matter near the surface of the earth may not be subject to considerable variation; though perhaps, taking large masses together, the density may be more uniform than is commonly imagined, except in hills that have been volcanos. The mountain Schehallien, however, bears not any appearance of having ever been in that state; it being extremely solid and dense, and seemingly composed of an intire rock.

rock. New observations on the attraction of other hills, would tend to procure us satisfaction in these points. But whatever experiments of this kind be made hereafter, let it be always gratefully remembered, that the world is indebted for the first satisfactory one to the learned zeal of the Royal Society, supported by the munificence of GEORGE THE THIRD.

Observations of Stars made with the zenith sector on the mountain  
Schellien, for discovering its attraction.

Result of Observations on the South-side of Schellien.												
Names of the Stars.	Point on the limb.	Limb of the Sector turned to the East.					Limb of the Sector turned to the West.					
$\alpha$ Cassio- pææ	24S	July 28. S 1.35,4::					Aug. 4. S 2.7,1	7. 5,5				
"	1S						Aug. 7. N 10,4	10. 12,1	15. 12,7			
$\beta$ Ursæ Major.	16N						Aug. 8. N 2.23,1					
$\gamma$	31S	July 29. N 2.16,9					Aug. 8. N 2.5,7 :					
$\epsilon$	9N	July 28 N 2.23,6										
$\zeta$	10S	July 28. N 1.11,5										
"	111S	July 22. N 19,8	28. 22,6									
$\delta$ Draconis	55N	July 26. N 2.26,0	28. 26,0				Aug. 4. N 2.14,0					
"	45N	July 26. N 4,2					Aug. 4. S 7,5					
"	96N	July 25. N 6,2	26. 5,4				Aug. 2. S 6,5	4. 5,4				
$\theta$	75S	July 21. N 1.37,4	24. 37,0	25. 36,1			Aug. 1. N 2.27,4	2. 24,7 :	4. 24,4	5. 26,9	6. 26,4	15. 28,7
$\kappa$	4N	July 20. N 3.4,5	21. 4,3	25. 5,4	26. 6,4		Aug. 1. S 9,2	15. 7,9				
$\nu$	92S	July 20. N 1.32,0:	25. 32,3				Aug. 5. N 1.20,7					
39	36N	July 24. N 19,1	26. 19,5	29. 19,8			Aug. 1. N 7,9	4. 11,3	15. 11,1			

Result of Observations on the South-side of Schehallien.												
Names of the Stars.	Point on the Limb.	Limb of the Sector turned to the East.						Limb of the Sector turned to the West.				
45 Draco- nis.	4N	July 21. N 1,0	29. 2,8					Aug. 1. S 9,4	4. 11,2	15. 5,6		
46	24S	July 24. N 23,9	26. 24,7					Aug. 4. N 13,6	10. 16,4	15. 16,6		
°	44N	July 21. S 4,2	25. 2,7	26. 2,2	28. 2,0	29. 1,0		Aug. 1. S 12,7	4. 12,4			
48	15N							Aug. 11 N 2,5,1	15. 6,0			
49	24S	July 29. N 2,28,0						Aug. 1. N 2,16,4	4. 17,6			
53	3S	July 26. S 22,2	28. 21,5					Aug. 1. S 32,0	4. 31,5	15. 28,4		
54	12N	July 29. S 18,4						Aug. 15. S 25,5				
α Cygni	66S	July 21. S 0,1	26. N 0,3									
'	97S	July 21. N 2,36,0	28. 35,0	29. 35,8	31. 36,5			Aug. 1. N 2,23,5	4. 22,8			
0	124S	July 26. S 19,6	31. 17,5					Aug. 1. S 31,4				
23	5N	July 24. N 8,5	29. 8,7	31. 11,0				Aug. 1. S 2,7	4. 2,4	7. 1,5	15. N 1,1	
33	14S	July 24. N 1,0,2	26. 0,5	29. 1,3	31. 3,1			Aug. 1. N 30,4	4. 30,7	7. 31,6	15. 34,9	
γ Cephei	77N	July 23. S 5,5	26. 2,4	29. 1,2	31. 1,6			Aug. 1. S 12,5	4. 11,2	7. 10,5	10. 9,8	15. 7,5
α	89N	July 23. S 25,0	26. 22,0	28. 20,8	29. 21,0	31. 20,0		Aug. 1. S 33,9	4. 32,8	7. 32,8	10. 31,7	15. 28,5
π Cygni	116S	July 29. N 12,8						Aug. 7. 0,0	10. N 1,9			

Result of Observations on the South-side of Sehehallien.										
Names of the Stars.	Point on the limb.	Limb of the Sector turned to the East.				Limb of the Sector turned to the West.				
10 Cephei	61N					Aug. 4. N 23,8				
12	44N					Aug. 12. N 21,9				
13 $\mu$	20S	July 26. N 34,0	29. 34,2			Aug. 7. N 24,7	10. 27,1	15. 28,3		
14	4N	July 26. N 2,38,5				Aug. 4. N 2,27,7	10 31,5			
15 $\mu$	37N					Aug. 12. S 24,3				
21 $\zeta$	8N	July 29. S 1,4,7	31. 2,9			Aug. 4. S 1,14,6	10. 11,5	15. 11,0		
	13S	July 23. S 27,8	26. 26,7			Aug. 4. S 36,0	10. 33,8			
27 $\delta$	11N	July 26. S 37,0	30. 36,9			Aug. 4. S 1,6,7	7. 6,7	9. 6,0	10. 5,5	15. 3,3
	149N					Aug. 9. N 1,3,5				
1 Cassio- peæ	27N	July 29. N 2,29,9	31. 30,3			Aug. 4. N 2,20,7	15. 22,9			
2	26N	July 31. S 13,0				Aug. 4. S 24,9	9. 23,5	15. 21,4		
5 $\tau$	13N					Aug. 10. N 16,2	15. 17,5			
7 $\epsilon$	8S					Aug. 10. S 2,14,4	15. 13,6			
8	22N	July 26. N 39,2				Aug. 4. N 29,0	7. 20,5	15. 31,9		

Result of Observations on the North-side of Schehallien.											
Names of the Stars.	Point on the limb.	Limb of the Sector turned to the West.				Limb of the Sector turned to the East.					
		Sept. 8. S 3,9,0	15. 7,4			Oct. 2. S 2,27,5	3. 25,3	5. 26,8	24. 22,3		
$\alpha$ Cassio- peæ.	24S										
"	1S	Sept. 7. S 33,8	8. 33,5	19. 30,3		Oct. 2. S 13,4	3. 11,8	9. 11,0	24. 7,2		
$\beta$ Ursa Major.	16N					Oct. 6. N 1,3,0					
$\gamma$	31S					Oct. 14. N 29,0	15. 25,5	18. 26,5	22. 26,4		
"	9N					Oct. 3. N 35,5	4. 35,6	15. 30,0	17. 33,1		
$\zeta$	10S	Sept. 15. S 24,2				Oct. 16. S 19,6	22. 22,0				
"	111S					Oct. 17. S 1,6,3	18. 6,6	22. 6,9			
$\delta$ Draconis	55N	Sept. 20, N 37,6				Oct. 17. N 1,6,1	22. 1,4				
$\theta$	45N					Oct. 17. S 1,13,7	23. 16,4				
"	96N	Sept. 7. S 1,18,0	20. 19,4			Oct. 23. S 1,12,3					
$\beta$	75S	Sept. 7. N 17,3	15. 17,5			Oct. 7. N 28,8	17. 27,2	23. 26,1			
$\xi$	4N	Sept. 15. N 1,29,5				Oct. 23. N 1,39,9					
$\gamma$	92S	Sept. 18. N 13,6	20. 13,4								
39	36N	Sept. 7. S 36,8	20. 38,9			Oct. 3. S 25,4	17. 26,5				
45	4N	Sept. 7. S 1,14,0				Oct. 17. S 1,1,1	23. 2,0				



Result of Observations on the North-side of Scheharhen.											
Names of the Stars.	Point on the Hub.	Limb of the Sector turned to the West.				Limb of the Sector turned to the East.					
46 Draconis.	24S	Sept. 7. S 33,0				Oct. 3. S 20,9	17. 13,7	23. 19,4			
"	44N	Sept. 18. S 1.10,5				Oct. 3. S 1.4,8	18. 3,3	22. 3,9			
49	24S	Sept. 20. N 1 12,2				Oct. 3. N 1.25,5	22. 26,5	23. 26,3			
53	3S	Sept. 7. S 1.35,0				Oct. 3. S 1.21,6	17. 20,0				
54	12N					Oct. 17. S 1.17,5	22. 18,0				
" Cygni	66S					Oct. 18. S 40,0					
"	97S	Sept. 7. N 1.20,0	18. 20,7	20. 20,5		Oct. 3. N 1.36,0	15. 35,9	18. 36,7			
"	124S	Sept. 7. S 1.34,6	20 33,7			Oct. 6. S 1.17,0	22. 17,6				
23	5N	Sept. 7. S 1.5,9	18. 3,9	20. 3,0		Oct. 3. S 29,3	15. 28,3	18. 28,6			
33	14S	Sept. 7. S 13,1	18. 10,3	20. 10,8		Oct. 3. N 4,0	6. 5,8	7. 5,8			
" Cephei	77N	Sept. 7. S 1.12,5	8. 13,1	18. 9,3	20. 10,6	Oct. 3. S 36,9	6. 35,1	7. 35,9			
"	89N	Sept. 7. S 1.33,6	8. 35,0	20. 31,9		Oct. 3. S 1.13,5	6. 12,0	23. 9,7			
" Cygni	116S	Sept. 7. S 1.1,1	8. 1,4			Oct. 6. S 21,5	9. 21,4				
10 Cephei	61N	Sept. 20. S 40,0									
13	26S	Sept. 7. S 17,5	18. 14,9			Oct. 3. N 1,5	7. 1,7	9. 3,5			

Result of Observations on the North-side of Schehallien.											
Names of the Stars.	Point on the limb.	Limb of the Sector turned to the West.				Limb of the Sector turned to the East.					
14 Cephei	4N					Oct. 9. N 2,9,8					
21 ζ	8N	Sept. 7. S 2.16,0	18. 11,7	20. 11,2		Oct. 3. S 1.35,6	6. 35,3	9. 33,5			
"	13S	Sept. 7. S 1.38,0	8. 37,3			Oct. 3. S 1.17,5	9. 15,0	23. 13,3			
27 δ	11N	Sept. 8. S 2.3,0				Oct. 3. S 1.30,6	6. 26,5	9. 25,5	22. 22,6	23. 22,6	24. 23,4
"	149N	Sept. 8. N 1,7	15. 3,8								
1 Cassio- pææ.	27N	Sept. 7. N 1.17,4	13. 21,2	15. 20,4		Oct. 3. N 1.39,0	6. 39,9	9. 20,0			
2	26N	Sept. 7. S 1.27,0	8. 26,0	15. 23,5		Oct. 3. S 1.4,5	6. 5,4	9. 3,9			
5 τ	13N	Sept. 7. S 29,4	8. 28,1	13. 26,4		Oct. 2. S 7,8	3. 7,8	9. 5,1			
7 ε	8S	Sept. 7. S 3.18,7	8. 18,5	15. 18,3		Oct. 3. N 1.38,0	6. 36,0	9. 40,5	24. 2,4,6		
β	22N	Sept. 7. S 14,0	8. 13,6	13. 11,7	15. 11,7	Oct. 3. N 6,9	6. 7,9	9. 8,9			

N. B. The figures set down under the days of the month are the revolutions and parts of the micrometer, by which the star is North or South of the division shewn in the second column. The revolution, though not repeated, is supposed to continue the same as in the first ob-

observation of the star. One part of the micrometer is  $= \frac{1}{1,013,754.5}$ , and one revolution or 41 parts is  $= \frac{1}{41,563.93}$ . 128 divisions of the arc have the chord  $= \frac{1}{8}$ th of the radius.

# P R E S E N T S

## MADE TO THE

# R O Y A L S O C I E T Y

In the Y E A R 1774;

W I T H

The N A M E S of the D O N O R S.

Donors.	Presents.
Omitted, 1774.	
Duc de Croy.	M. de Vaugondy's Map of the Southern Hemisphere.
Nov. 10. Mr. Hope of Amsterdam.	Transactions of the Harelem Academy of Sciences, 14 vol. 8°
R. Ac. of Sciences Paris.	Memoires for the Years 1760 and 1771, 2 vol. 8°
Abbe Rozier.	Journal, &c. Physique, for May, June, July, 1774. 4°
John Strange, Esq. F. R. S.	Zanetti Statue d'Afclepiade d'Abano. 4°
<hr/>	Discorsi Epistolari sopra i fuochi di Loria. 4°
Coll. Phys. Edinburgh.	Pharmacopœia, 1774. 1°
The Hon. C. J. Phipps, now Lord Mulgrave, F.R.S.	A Voyage towards the North Pole, in 1773. 4°
Samuel Hartley, LL.D. Sec. R. S.	Remarks on the Observations made in a late Voyage towards the North Pole. 4°
J. B. Catp. d'Anfi de Villoifon,	Apollonii Sophistæ Lexicon Homericum, 2 vol. 4°
W. Gerard de Brahm.	The levelling Balance and Counter-balance, fol.
M. l'Abbe Marie.	Traité de Mechanique, 1774. 4°
17, Samuel Wegg, Esq. V. P. R. S.	Three rare Birds, some Fungus and Moss from Hudson's Bay, with some Account of them by Humphry Martin, Resident there.

Donors.

1774.

Donors.

Presents.

- Dec. 1. William Hunter, M.D. F. R. S. Anatomy of the human gravid Uterus, 1774. Atl.  
 M. Portal. Rapport sur la mort du Sieur le Maire, &c. par la vapeur du Charbon. 8°  
 M. Sanchez, M. D. Examen sur l'apparition de Maladie Venerienne en Europe. 8°  
 Leonard Euleri. Novæ Tabulæ Lunares. Petrop. 8°  
 15. M. Necker. Methodus Muscorum, per classes, &c. 8°  
 Richard Twiss, Esq. F. R. S. Phyllologia Muscorum per examen analyticum. 8°  
 22. R. Ac. Sciences Paris. A Snake-stone from Spain.  
 Ow. Salus. Brereton, Esq. F.R.S. Connoissance des temps, pour 1775. 8°  
 Alexander Hunter, M. D. F. R. S. A Zoophyte, found in the Sea near Scarborough.  
 M. Aublet. An engraving of an ancient Gold Ring.  
 Hudson's Bay Committee. Two new Plates, with their drawings and descriptions in Latin and French.  
 Several dried Animals from Hudson's Bay.  
 A piece of a Tree, Eight Inches diameter, cut down by the Beavers near Hudson's Bay.

1775.

- Jan. 12. Matthew Raper, Esq. F. R. S. Dr. Bradley's Quadrant Observations, MS. fol.  
 Dr. Bradley's Transit Observations, MS. fol.  
 These are copies by the late Gael Morris.  
 An uncommon fish salted, with a drawing of it.  
 A Sea-cow's Head, and Two Fœtus' of that Animal, in Spirits.  
 Several Caperlines, the Bait for Cod, in Spirits.  
 A large Loon diver dried.  
 A musical Instrument from the Island of Amsterdam in the South Seas.  
 A new colouring Substance, from ditto.  
 A Print of Don G. Juan, F. R. S.  
 Exposition des differens Methodes d'administrer le Mercure, &c. 8°  
 MS. Dissertation on the Insufficiency of the Signs from Urine.

Donors.

1775.

Donors.

Presents.

- M. de Horne.
26. Alex. Dalrymple, Esq. F. R. S.
- Peter Mullman.
- Feb. 2. John Hunter, F. R. S.
9. Thomas Henry, F. R. S.
16. Society of Antiquaries.
- 
23. J. B. B. Grosse.
- Thomas Tyrwhitt, Esq. F. R. S.
- March 2. Samuel Vernon.
9. John Ellis, Esq. F. R. S.
- M. de Vaugondy.
16. Sir John Pringle, Bart. F. R. S.
23. Samuel Foart Simmons.
- Mr. Hope of Amsterdam.
- Apr. 6. Richard Twiss, Esq. F. R. S.
27. Commissioners of Longitude.  
The Editor.
- Edward Jacob.
- M. Gibelin, D. M.
- J. Muller.
- May 4. P. G. F. Fontana.

- MS. Observations on a Uterus without a Cavity.
- Plans of Ports in the East Indies, N<sup>o</sup> III and IV, fol.
- A new and complete History of Essex, in 6 vols. 8<sup>o</sup>
- A Preparation of the Gymnotus, or Electrical Eel, in Spirits.
- Experiments and Observations on Six Subjects, Medicinal, &c. 8<sup>o</sup>
- A Print of an ancient Picture in Windfor Castle.
- An Historical Description of the Print, by Sir Joseph Ayloffe, Bart. F. R. A. S. 4<sup>a</sup>
- Recueil des Antiquités et Monumens Mar-seillois. 4<sup>a</sup>
- The Canterbury Tales of Chaucer, with an Essay, Notes, &c. 4 vols. 8<sup>a</sup>
- Meteorological Observations, made at Middlewich in Cheshire, MS. from 1768 to 1771, inclusive.
- A Description of the Mangostan, and of the Bread Fruit 4<sup>a</sup>
- Memoire sur les pays de l'Asie et de l'Ame-rique au N<sup>o</sup> S. S. 4<sup>a</sup>
- A Sepia, or Ink Fish, preserved in Spirits.
- Element of Anatomy and the Animal Oeco-nomy. 8<sup>a</sup>
- Memoires of the Society of Sciences of Haer-lem, vol. XV. 8<sup>o</sup>
- Travels to Portugal and Spain. 4<sup>a</sup>
- The Nautical Almanack for 1776. 8<sup>o</sup>
- The Probability of reaching the North Pole discussed. 4<sup>a</sup>
- The History of the Town and Port of Fe-verham in Kent. 8<sup>o</sup>
- Experiences et Observations sur differentes espèces d'Air. 8<sup>o</sup>
- Botanical Prints, N<sup>o</sup> II. fol.
- Delle altezze Barometriche faggio Analitico. 4<sup>o</sup>

	Donors.	Presents.
1775.		
18.	John Ellis, Esq. F. R. S.  Peter Chester. University of Gottingen. John Hunter, F. R. S.	The Fruit and Blossoms of the Theobroma Cacao, in Two Jars of Spirits. Some Slices of Jalap from Florida. Tobias Mayeri, Opera inedita. fol. A slice of the Gymnour Electricus, in a Jar of Spirits.
25.	R. Lowth, Bp. of Oxford, F.R.S.	De sacrâ Poësi Hebræorum, Edit. tertia. 8°
June 1.	L. Visc. Mahon, F. R. S.	Considerations respecting Gold Coin. 4°
15.	Abrah. D'Aubant,  Count de Salis, F. R. S.	The Works of William Thomas, Clerk of the Privy Council 1549. 8°
29.	W. Bowyer and J. Nichols.  _____  _____  _____	La sacrâ Biblia, in Lingua Romanesca. fol. Remarks on a late Dissertation on the Greek and Roman Money. 4° Lud. Kusterus De vero usu verborum me- diorum. 8° Occasional Discourses in the Royal Navy, in 1756, 57, and 59. 8° Select Discourses on the Hebrew Months, Sabbatical Year, &c.
	Dionis du Sejour, F. R. S. John Wynn Baker.	Essai sur les Comètes en general, &c.
July 6.	R. Acad. Stockholm.  _____  _____	Experiments in Agriculture in 1772 and 1773. 8° De Geer. Memoire pour l'Histoire des In- sectes, tom. IV. 4° Nova Acta Regiæ Societatis Scientiarum Up- saliensis. 4° Swedish Acts, Two Numbers for 1773, and Two Numbers for 1774. 8°
	John Harrison, F. R. S.  _____	On Clock-work and Music, 1775. 8°
	M. Messier, F. R. S.	Instructions sur les Maladies que détruit le Bétail. 4°
	J. H. de Magellan, F. R. S.	Observations Meteorologiques faites à Peking, par P. Ansat. 4° Description des Océans et Sextants Anglois. 4°

# E R R A T A.

Page Line

- 121. 3. from the bottom, *for* 280° *read* 270°
- 251. 2. *for* has *read* have
- 262. 22. *for* grafs and flies *read* gnats
- 267. 2. *for* caves *read* caves
- 12. *for* when she stretches *read* and stretches
- 328. 14. *dele* and
- 341. 1. from the bottom, *for* sufficient *read* sufficient
- 464. 19. *for* iuconuenience *read* inconvenience





A N  
I N D E X  
T O T H E  
S I X T Y - F I F T H V O L U M E  
O F T H E  
P H I L O S O P H I C A L T R A N S A C T I O N S .

A.

- ABANO*, an extraordinary animal in the waters of, p. 45, 46. Heat of the baths there, *ibid*.
- Abyssea*, observations made there upon myrrh, in the year 1771, p. 408. See *Myrrh*.
- Accephalous* birth, an account of an extraordinary one, p. 311. See *Birth*.
- Acid*, mineral, a new one produced from phosphoric spar, p. 394.
- Alma*, conjectures concerning, p. 29.
- Akworth*, in the county of York, average of deaths there for ten years, p. 428. Christenings and burials for twenty years, p. 443, 444.
- Air*, fixed, apparatus for impregnating water with, description of one, p. 59. See *Water*.—*Air*, an account of further discoveries in, p. 384. The vitriolic acid, the nitrous acid, and the vegetable acid air, discovered, p. 385. Vitriolic acid air how produced, *ibid*. Vegetable acid air how produced, p. 386. No fluid capable of confining nitrous acid air, *ibid*. Kinds of air formed by the nitrous acid more numerous than all the kinds that can be formed by the other acids, *ibid*. How produced, p. 386, 387. Various metals yield inflammable air by the foregoing process, p. 387. Air, five or six times better than common air, p. 387—389. How produced, p. 387. Conjecture that the nitrous acid is the basis of common air, p. 389, 394. Pure air procured from a variety of materials, p. 390. Fixed air a modification of the nitrous acid, *ibid*. A pure *dephlogisticated* air may be procured from earths of all denominations, p. 392. Observations on air, p. 392, 393. Air produced from ore of lead and spirit of nitre, p. 393.

*Allahabad*, in the East Indies, an account of some thermometrical observations made there, in lat.  $25^{\circ} 30'$  N. during the year 1767, and also during a voyage from Madras to England in the year 1774, p. 202. — Natural ice seldom seen there, p. 252.

*Altar*. Prognostics derived from the bright Star, 182.

*Amputation* above the knee, remarks and considerations relative to the performance of it by the single circular incision, p. 373. The manner of operation, p. 373—375. Remarks on amputation, and the single incision recommended, p. 375—377.

*Amsterdam*, tile of, in the South Seas, account of a musical instrument brought from thence, p. 67. See *Musical Instrument*. — Remarks on a larger system of reed pipes from thence, with observations on the nose flute of Otahcite, p. 72. — Experiments on a new colouring substance from thence, p. 91. See *Colouring Substance*.

*Andes*. See *Vokanos*.

*Anemonies*. See *Sea-Anemonies*.

*Aneurysms* in the thigh, concerning, p. 378—380.

*Animals* and vegetables, experiments on, with regard to the power of producing heat, p. 446. See *Heat*. Animals, their power of generating cold, p. 112, n. 121. Their power limited, p. 121. Animals must be deprived of life before they can be frozen, p. 452. On what their power of producing heat depends, p. 453, 457. Animals, imperfect, allow of a considerable variation in their internal temperature, *ibid*. Torpidity of animals in winter, accounted for, *ibid*. Animals and vegetables, in what they differ, p. 456.

*Animals* influenced by the approaching changes of weather, p. 246.

*Aratus*, the concluding lines of his *Διοσημενα*, p. 186. He derives prognostics from the position of the moon's horns, p. 182.

*Arqua*, beautiful marble found near Arqua, p. 35. Relicks of Petrarch and Laura, *ibid*.

*Artery* Femoral, a division of it into two trunks, p. 378. Experiments upon a Dog, p. 380—383.

*Astronomical Observations*. See *Leicester*, *Occultations*, *Schehallien*.

*Attraction*, a proposal for measuring that of some hill in this kingdom, by astronomical observations, p. 495. An attempt of this kind on the mountain Chimborazo, p. 496, 497. Why the observations were insufficient to prove the reality of an attraction, p. 497. Attempts to find convenient places in this kingdom for the trying such a curious experiment, p. 498, 499. — Account of some observations made on the mountain Schehallien, for finding its attraction, p. 500. See *Schehallien*.

*Auvergne*, in France, remarkable for basaltine hills, p. 9—11, 14—16, 19, 21. A city there built on a giants catfeway, p. 15, 16.

## B.

*Bank Martin*, account of the, p. 272. See *Sand Martin*.

*Barker*, Sir Robert, has account of some thermometrical observations made at Allahabad in the East Indies, in lat.  $25^{\circ} 30'$  N. during the year 1767, and also during a voyage from

- from Madras to England, in the year 1774, p. 202. See *Allababad*. — His account of the process of making ice in the East Indies, p. 252. See *Ice*.
- Barker*, Mr. Thomas, his extract of a register of the barometer, thermometer, and rain, at Lyndon, in Rutland, for the year 1774, p. 199. See *Lyndon*.
- Barometer*, state of it at London, throughout the year 1774, p. 140—164. Influence of the winds upon, p. 175. — State of it at Bristol for the year 1774, p. 194 — At Lyndon in Rutland, p. 199. — At Chislehurst in Kent, p. 294. — Sea anemonies commended to form one, p. 240, 245, 246.
- Basaltes*. Two Basaltine Hills in the Venerian State, p. 6. See *Rosso* and *Diavolo*. Their columns, the *corneus crystallifatus prismaticus lateribus inordinatis Wallerii*, p. 7. Description of the columns, *ibid*. The hexagonal form chiefly prevails in columns of basaltes, p. 10. Basaltine groups generally have an affinity with the mass of the Hill to which they belong, p. 10. Basaltine columns of Monte Rosso bedded in a mass of Granite, p. 10. Basaltine columns observed near Auvergne, le Mont d'Or, and St. Alcon, *ibid*. Columns of Monte Rosso and San Giovanni preserve a position nearly parallel, p. 11. The like parallelism observed in other groups, p. 19. Varieties of position generally observable in the different groups of the same hill, p. 11. The horizontal position not excluded, p. 12. Remarks on the basaltes of Auvergne and Velay, p. 13—16. Basaltine columns contained in the Welsh Mountains, p. 16. Of the origin of basaltine columns, p. 17—46. Not produced by deposition from an aqueous fluid, p. 17. Not parasitical bodies, p. 18. Form regular strata, p. 11, 12. 19. Arguments of their igneous origin, p. 420. No instance of a *Crater* in any basaltine hill, p. 21. Basaltine columns near *Gambellara*, p. 44. Near Mason, *ibid*. Near San Giovanni Marione, *ibid*. The Basaltine columns of il Sasso di san Biafo, p. 419. Basaltes en tables, p. 421.
- Bats*, frequently found in a state of torpidity, p. 346.
- Bean*, experiments on its power of generating heat, p. 450—452.
- Bees*, heat of a swarm, p. 122.
- Bengal*, general state of the weather there, p. 205. Rains cause great floods there, p. 206. Waters become putrid, and bring on the *pucker fever*, *ibid*.
- Bitch*, experiments on one, in an heated room, p. 489.
- Birds*, several proofs of their torpidity, p. 348—350.
- Birth*, acephalous, an account of an extraordinary one, p. 311. Anatomical description of it, p. 313—315. Observations thereon, p. 316—321.
- Black*, Dr. Joseph, his experiments ascertaining the supposed effect of boiling upon water, in disposing it to freeze more readily, p. 124. See *Water*.
- Black Martin*, account of the, p. 264. See *Swift*.
- Blagden*, Dr. Charles, his experiments and observations in an heated room, p. 111. 484.
- Body*, animal, has a power of destroying heat, p. 117—121.
- Boiling* disposes water to freeze, p. 124. 253. 255. Conjecture concerning the cause, p. 127.

- Bolca*, a fossil fish quarry there, p. 25.  
*Brain*, children born with little or none, have the *capsule renales* very much diminished, p. 315. *n.* An instance against the common persuasion that brainless children are remarkably brisk before they are born, p. 317.  
*Bristol*, extract of a meteorological journal kept there, for the year 1774, p. 194. See *Meteorological Journal*.  
*Brownside*, in Derbyshire, number of its inhabitants in the year 1773, p. 326.  
*Bruce*, Mr. James, his observations upon myrrh, made in Abyssinia, p. 408. See *Myrrh*.  
*Buccinum*, a species of, an extraordinary animal in the waters of Abano, p. 45, 46. Vau-delli's figure of this animal, inaccurate, p. 46.  
*Bugsworth*, in Derbyshire, number of its inhabitants in the year 1773, p. 326.  
*Bushel*. See *Weights and Measures*.

## C.

- Cader Idris*, a mountain in Wales, prismatic basaltine columns on the top of, p. 47.  
*Calcutta*, in the East Indies, natural ice never discovered there, p. 352.  
*Cancer*, Prognostics derived from stars in that constellation, p. 182.  
*Capsule renales*, very small in children born with little brain, p. 315. *n.*  
*Carp*, experiments on their power of resisting external cold, p. 122. 447.  
*Caucalis* of the ancients, what, p. 411.  
*Chester*, observations on its bill of mortality for the year 1773, p. 85. Healthy to a remarkable degree, *ibid.* Uncommon instances of longevity there, *ibid.* Women live longer than men, p. 86. No epidemic in it this year, *ibid.* Remarks on inoculation and the small pox, p. 87. Between the ages of 15 and 50, more died of consumptions than of all other diseases, *ibid.* Deaths, ages, and conditions, p. 88. Diseases of different ages, p. 89. Diseases of different months, p. 90. General bill of marriages, baptisms, and burials, *ibid.*  
*Children*, great towns fatal to, p. 324.  
*Chillac*, town of, in Upper Auvergne, built on a giants causeway, p. 16.  
*Chimborazo*, the mountain, an attempt to measure its attraction by astronomical observations, p. 496, 497.  
*Chinley*, in Derbyshire, number of its inhabitants in the year 1773, p. 326.  
*Chorobent*, in Lancashire, survey of its inhabitants in the year 1773, with the state of births and deaths, p. 431, 432, *n.*  
*Chislehurst*, in Kent, state of the thermometer there for the year 1774, p. 294.  
*Circular Arc*, time of descent in, p. 287, 288.  
*Clock*, observations on the going of one, p. 290.  
*Cocky Moor*, in Lancashire, survey of its inhabitants in the year 1773, with the state of births and deaths, p. 431, *n.*  
*Cold*, animals capable of generating, p. 112, *n.* of resisting it, p. 446. See *Heat*.  
*Colouring Substance*, from the island of Amsterdam, in the South Seas, p. 91. Of a light bright orange colour, *ibid.* Gives a yellow stain to the skin not readily washed out

with

with soap and water, *ibid.* Experiments made thereon, p. 91—93. Only of use in dying silk and wool, p. 93. No colours dye so speedily, and few go farther, *ibid.* Of the resinous kind, and of affinity with *annotta*, *ibid.*

*Concretions*, volcanic, discovered in the Venetian State in Italy, p. 5. See *Basaltus*.

*Constellations*, influences ascribed to them by the ancients, p. 184. Whence the opinion of their influence arose, *ibid.* Why it does not now prevail equally with the opinion of the moon's influence, *ibid.*

*Cooper*, Dr. W. his account of an extraordinary acephalous birth, p. 311. See *Birib*.

*Cornish*, Mr. James, his account of the torpidity of swallows and martins, p. 343—352. See *Torpidity*.

*Cossavo*, volcanic hills in the neighbourhood of, p. 34.

*Crater*. No instance of a Crater in any basaltine hill, p. 21.

*Curve*. Elastic curve, and Curve of Equable recess, both constructed by the rectification of the Ellipsis, p. 289.

## D.

*Darwen*, in Lancashire, survey of its inhabitants in the year 1773, with the state of births and deaths, p. 431, 2.

*Deaths*, proportion of to marriages in different places and at different periods, p. 328, 329. See *Life*.

*Derham*, Mr. his calculation of the proportion of male to female births, p. 332. 434. His calculation confirmed, p. 333. 435.

*Diavolo*, il monte del, a basaltine hill in the Venetian State, p. 6. A topographical view of it, Tab. 2. Situation of this causeway, p. 24. The basaltine columns of il monte del Diavolo, bedded in volcanic sand, p. 12. See *Basaltus*, *Hills* *Volcanic*.

*Diequemare*, Abbé, his second essay on the natural history of the sea anemonies, p. 207. See *Sea Anemonies*.

*Dipping-Needle*, description of a new one, p. 79. Dipping-needles ill calculated, and a new one contrived, p. 80. Description of it, p. 80, 81. Its use and management, p. 81, 82. Dipping-needles, to be of use at sea, how to be placed, p. 82. Method of setting them at sea inaccurate and untrue, p. 82, 83. A constant power in this instrument of setting itself in the proper position, of keeping in it, or of restoring itself to it in case it has lost it, p. 83. By its double motion, counteracts the rolling of the vessel, *ibid.* Method of examining the observations, *ibid.* A small difference occasioned by it, p. 84. Queries concerning this variation, *ibid.* — Dipping-needle, experiments on the, p. 129. Observations at Stromness, in the isles of Orkney, p. 130, 131. In Hudson's bay, p. 132—136.

*Dobson*, Dr. his experiments in an heated room, p. 463.

*Dormice*, frequently found in a state of torpidity, p. 346. Experiments on their power of resisting external cold, p. 448, 449.

## E.

*East Indies*, the process of making ice there, 252. See *Ice*.

*Edinburgh* and *London*, a comparison of the heat of, 459. See *Heat*. Comparative heat of *Moscow* and *Edinburgh*, p. 460.

*Eel*, electrical, an account of one, 102. Experiments and observations on one, 94. See *Gymnotus Electricus*.

*Electricity*, a shock of, cannot be communicated by a chain, or where there is the least interruption of continuity, p. 3, n.—To kill a whole plant by, 456, n.

*Evaporation*, a powerful frigorific cause, but not supposed sufficient to keep the living body cool in air heated to a very great degree, p. 492.

*Euganean hills*, chiefly formed of *granitello*, p. 9. Their situation, extent, and external characters, p. 33—36. Few of them consist of common lava, p. 35. Particular descriptions of some few which do, p. 38—41. An account of a curious giants causeway newly discovered there, 418. See *Basaltus*.

*Experiments* and observations in an heated room, 111, 463, 484. See *Room*.

*Experiments* on a new colouring substance from the island of *Amsterdam* in the *South Seas*, 91. See *Colouring Substance*.—On the supposed effect of boiling upon water, in disposing it to freeze more readily, 124. See *Water*.—On the Dipping-needle, 129. See *Dipping-Needle*.—On animal and vegetables, with respect to the power of producing heat, 446. See *Heat*.

*Experiments*, with the ore of lead and spirit of nitre, 393. In an heated room, 463. Electrical, with the *gymnotus electricus*, 96—101, 107, 108. On a musical instrument brought from the isle of *Amsterdam*, in the *South Seas*, 67. Astronomical, 509, &c. On sea anemones, 207, &c. On the torpedo, 1.

## F.

*Farr*, Dr. Samuel, his extract of a meteorological journal for the year 1774, kept at *Bristol*, p. 194. See *Meteorological Journal*.

*Fatt*. See *Weights and Measures*.

*Females* and males, number born at different places and at different periods, 333, 435.

*Females* more than males baptized, at *St. Stephen's Bristol*, for 20 years, 332. Fewer born than males, 435. More living than males, 335, 435, 436. Longer-lived than males, 436.

*Fever*. Pucker fever. See *Bengal*.

*Fire*, *Scottish*, experiments on its power of generating heat, 451, 452.

*Fire*, its operation on lapideous solids, p. 26.

*Fish*, killed by an electrical fluid from the *gymnotus electricus*, 96, 97, 98.

*Flute*, *note*, of *Otaheite*, p. 72. Remarks on the sounds produced by it, p. 72—74. Sounds noted to consist pitch, p. 72, 73. Capable of several different melodies, though chiefly melancholy, p. 73.

*Fossil-Fish*, quarry near Bolca, p. 52.

Freezing of water hastened by previous boiling. See *Water*.

*Frog*, experiments on the variations of its internal temperature, p. 453.

*Frost*. Frosty days at London in the year 1774, p. 173.

*Fruits*, several kinds, which generally come to maturity near London, never ripen in the neighbourhood of Edinburgh, p. 456. Why, p. 460.

## G.

*Gallon*. See *Weights and Measures*.

*Ganges*, season and supposed cause of the River's swelling, p. 206.

*Garden*, Dr. Alexander, his account of the *gymnotus electricus*, or electrical eel, p. 102.

See *Gymnotus Electricus*.

*Giants* causeways, an account of two discovered in the Venetian State in Italy, p. 5, &c.

See *Basaltus*, *Rosso*, *Diavolo*.—Giants causeway in Ireland, its columns approach a circular form, p. 7. Their width, *ibid*. The principal aggregate perpendicular to the horizon, *ibid*.—Giants causeway, or group of angular columns, an account of

a curious one newly discovered in the Euganean Hills, near Padua in Italy, p. 418.

Situated at Castel Nuovo, on the rock *Il Sasso di San Biafo*, S. W. of Monte Rosso, *ibid*. The columns and rock composed of a sort of grey granite, common to the Euganean hills, p. 419. Observations on columnar crystallizations, p. 419. 423.

*Glass*, fossil, found near Monte Nuovo, and in the Volcanos of the Andes, p. 40.

*Gooch*, Mr. Benjamin, his remarks and considerations relative to the performance of amputation above the knee, by the single circular incision, p. 273. See *Amputation*.—

On aneurysms in the thigh, p. 378. See *Aneurysms*.

*Granite*, in many places intermixed with basaltine and other volcanic hills, p. 14.

None found in the volcanic hills of the Vicentine and Veronese district, p. 25.

Analogy between Granites and Volcanic concretions, p. 41. Parallel strata of Granite near the top of St. Gothard, p. 422.

*Granitello*, an inferior kind of Granite, which abounds in some parts of France and Italy, p. 9. The bulk of the Euganean hills principally consists of it, *ibid*. The

Granitello of Monte Rosso contains common lava, p. 36. Orbicular and laminated form of the Granitello of the Euganean hills, p. 40. 41.

*Guetard*, M. his principle, that all volcanic materials observable in calcareous countries are adventitious, refuted, p. 43.

*Gun-powder*. Air produced by its explosion, p. 393.

*Gymnotus Electricus*, or electrical eel, experiments and observations on, p. 94. Eel

fickened by change of climate, *ibid*. Kept in a large vessel, and the experiments made while the fish was in the water, p. 95. Two persons, not in contact, receive an electrical shock through the water, *ibid*. Powers greatly superior to the torpedo, *ibid*.

Caught in the province of Guiana, westward of Surinam, *ibid*. The *gymnotus* has the power



power of electrifying, and of killing its prey at a distance, p. 95, 97. Description of it, p. 96. The effects of its shock compared with those of artificial electricity, p. 96—101. The gymnotus kills small fishes, and swallows them, *ibid.* He kills a cat-fish, but cannot swallow it, *ibid.* Those fishes killed by an emission of electrical fluid, *ibid.* A farther proof of it, p. 97, 98. Though apparently dead, they recover when removed into other water, p. 98. Fish stunned by a small electrical shock recover in the same manner, *ibid.*—Gymnotus Electricus, an account of one, p. 102. Brought from Surinam, p. 102, 109. Particular description of it, p. 103, 107. Amphibious, p. 106. Its amazing power of giving an electrical shock to any number of persons, p. 107. Power stronger when first caught, *ibid.* The shock received by taking the fish in both hands, p. 108. Also by touching it with one hand, and putting the other in the water over its body, *ibid.* The same effect on a number of persons joining hands, and at the extremities of the circle one touching the fish, the other putting his hand in the water over the body of it, *ibid.* Differs from the torpedo, *ibid.* The shock of the gymnotus wholly electrical, *ibid.* Communicated by the same conductors, and intercepted by the interposition of the same original electrics, p. 109. A fresh-water fish, *ibid.* 96. Without teeth, *ibid.* Mr. Bancroft's account of this animal not very particular, p. 110. Electrical eels twenty feet long; a shock from them said to cause instant death, *ibid.*—An account of one by Mr. Hunter, p. 395, 407. The organs which produce the peculiar effect of the fish described, p. 396, 399. Of the structure of these organs, p. 399, 402. Of the nerves in this animal, p. 402, 404. Of its blood vessels, p. 404, 405. Explanation of plate I. fig. 1. and 2. p. 405. Of plate II. fig. 1. p. 405, 406. Of plate III. fig. IV. p. 406, 407. Of plate IV. fig. 5. p. 407.

## H.

*Haffenden*, Mr. his account of the effects of lightning on a house, which was furnished with a pointed conductor, at Tenterden, in Kent, p. 336. See *Lightning*.

*Hale*, in Lancashire, survey of its inhabitants, in the year 1773, with the state of births and deaths, 431, 2.

*Haygarth*, Dr. his observations on the bill of mortality for Chester for the year 1775, p. 85. See *Chester*.

*Heathcote*, striking disparity of, in the town of Manchester and in the country round it, p. 323.

*Heat*, Experiments on animals and vegetables, with respect to the power of producing, p. 426. Animals capable of resisting external cold while alive, *ibid.*—Heat, a comparison of that of London and Edinburgh, p. 459. The summer-heat of London exceeds that of Edinburgh more in proportion than the winter heat, p. 460. The reason of this, *ibid.* Comparative heat and cold of Moscow and Edinburgh, *ibid.* Connection between the heat of a country and the heat of its springs, p. 461.

*Heat*, doubts about the common theory of the generation of it in the living animal body, 122.

*Heated*

- Heated Air.* Experiments on the effects of air heated to a great degree on the living human body, p. 113—199, 463, 464, 484. Raises the pulse, p. 114, 115, 118, 464, 486. The Thermometer exposed to air highly heated, sinks upon being applied to the palms of the hands, or under the Tongue of a person who has continued many minutes in the same air, p. 114, 115, 118. The living human body supports air heated to  $211^{\circ}$ , p. 117, to  $224^{\circ}$ , p. 464, to  $260^{\circ}$ , p. 481. The heated air put in motion intolerable, p. 487. Effects of cloaths in enabling the living human body to bear such high degrees of heat, p. 487. Singular sensations produced by these high degrees of heat, p. 118, 485. Degrees of heat supportable by the living body very different in different media, p. 119. Experiments on the white of an egg in air heated to  $224^{\circ}$ , p. 465. On bees-wax in the same air, p. 466. Experiment on a dog in air heated to 220, p. 489. Experiments on sundry inanimate substances in these great degrees of heat, 491, 492.
- Hecla*, conjectures concerning, p. 30.
- Helvettin* and *Skidda*, height of those hills above the intermediate valley, p. 498.
- Henley*, Mr. his remarks on an account of the effects of lightning on a house, which was furnished with a pointed conductor, at Tenterden, in Kent, p. 344, 342. See *Lightning*.
- Hills.* Hills Vulcanic. Not generally produced from the bowels of the Earth by subterraneous explosions, p. 20, 27.
- Horizontal vulcanic hills, p. 22.
- Vulcanic Hills of the Vicentine and Veronese district described. Abound with limestone and lava, p. 24. Without any mixture of Granite, p. 25. Original structure and characters of these hills, before they were changed by fire, still evident, *ibid*.
- Vulcanic Hills near Cossovo in Dalmatia, p. 34. In the county of Down in Ireland, *ibid*.
- Hirundines*, British. See *House-Swallow*, *Swift*, and *Sand-Martin*.
- Horsley*, Rev. Dr. his abridged state of the weather at London in the year 1774, collected from the meteorological journal of the Royal Society, p. 167. See *Weather*. De polygonis area vel perimetro maximis et minimis, inscriptis circulo, vel circumculum circumscribentibus, p. 301—310. Vide *Theorema* I, II, III, IV, V, VI.
- Horwich*, in Lancashire, survey of its inhabitants, in the year 1773, with the state of births and deaths, 431, &c.
- House-Swallow*, account of the, p. 258. The first comer of the British *hirundines*, *ibid*. Appear about the middle of April, about lakes and mill-ponds, *ibid*. Their withdrawing for a time if they find frost and snow, more in favour of hiding than migration, p. 258, 259. Build their nests in various places, p. 259. Nests described, p. 259, 260. Dam submits to an inconvenient situation to secure her brood, p. 260. What number of eggs they lay, their colour, and time of hatching, *ibid*. Progressive method of introducing their young into life, p. 260, 261. The first brood congregates with the first brood of house-martins, and the dam begins the business of a second

second brood, p. 261. When the second brood are produced, *ibid.* The swallow an instructive pattern of industry and affection, *ibid.* Resorts where insects most abound, *ibid.* The motion of the mandibles when a fly is taken too quick for the eye, but the snap of the bill distinctly heard, *ibid.* The male swallow probably the *excubitor* to the house and other small birds, *ibid.* He announces the approach of birds of prey, when all the swallows and martins join in a body, and buffet their enemy from the village, p. 261, 262. He also sounds the alarm, and strikes at cats, p. 262. Each species of *hirundo* drinks as it flies, *ibid.* The swallow alone washes on the wing, *ibid.* Swallow a delicate songster, and bold ranger, *ibid.* Small parties often attend horsemen, flying around and collecting insects roused by the horses feet, *ibid.* Their food, *ibid.* Before their departure they all forsake houses and chimnies, and roost in trees, *ibid.* Withdraw about October, *ibid.* Some few parties haunt the open streets of London next the fields, *ibid.* How distinguished from their congeners, p. 263. The most nimble of all the species, *ibid.* Want of sagacity, *ibid.* See *Swift, Sand-Martin.*

*Loy*, a mountain near Stromness in the Orkneys. Its height, p. 138.

*Hunter*, Mr. John, his experiments of the *gymnotus electricus*, p. 395—407. See *Gymnotus Electricus*.—His experiments on animals and vegetables, with respect to the power of producing heat, p. 446. See *Heat*.

*Hutchins*, Mr. Thomas, his experiments on the dipping-needle, p. 129. See *Dipping-needle*.

*Hygrometer*, state of the, for the year 1774, at Chislehurst, in Kent, p. 294.

*Hyperbola*, rectification of, p. 283—287.

## I.

*Ice*, the process of making it in the East Indies, p. 252. Natural ice never discovered at Calcutta, and but seldom at Allakabad or Mootegil, 252. 255. Methods of making ice, p. 252, 253. Remarks on its congelation, p. 254—256.

*Ingenhousz*, Dr. John, his experiments on the torpedo, p. 1. See *Torpedo*.

*Angleborough*, its height, p. 498.

*Inoculation*, remarks on, p. 87.

*Instrument*, musical, an account of one brought from the isle of Amsterdam, in the South Seas, p. 67. See *Musical instrument*.

*Journal*, meteorological, for the year 1774, kept at the Royal Society's house. See *Tables*.—One kept at Bristol for the year 1774, p. 194. See *Meteorological Journal*.

*Irritation*, a conjecture that it depends on the gelatinous part of the muscular fibre, p. 210. Our ignorance about it, p. 213.

## L.

*Landen*, Mr. John, his investigation of a general theorem for finding the length of any arc of any conic hyperbola by means of two elliptic arcs, p. 283—286. Some other

- other new and useful theorems deduced therefrom, p. 286—289. Observations thereon, p. 289.
- Lava* abounds in the hills of the Veronese, p. 24. And in those of Auvergne and Velay, p. 35. More rare in the Euganean Hills, p. 35. Found in the Granite of Monte Rosso, p. 36. Singular kind of Lava of which Monte Nuovo is composed, p. 38. Lava brecciata, *ibid.* Lava-pebbles observed in quantity in the beds of the rivers flowing from the mountains near Friuli, p. 43.
- Leicester*, latitude of, p. 366.
- Level*, Mr. an extract of his letter from Petersburg, concerning the occultations of the fixed stars by the moon, p. 280. Two curious theorems, by means of which the solution of polygons will be as easy as that of triangles by common trigonometry, p. 281, 282.
- Life*, has a power of producing or destroying heat, 112, 122.
- Life*, human, observations on the difference between the duration of it, in towns and in country parishes and villages, p. 424. The proportion of inhabitants dying annually in great towns, from 1 in 19 to 1 in 22 or 23; in moderate towns, from 1 in 24 to 1 in 21; in country parishes and villages, 1 in 40 or 50; p. 426. Average of deaths at Stockholm for six preceding years, 1 in 19 annually, *ibid.* At Rome for ten years 1 in  $21\frac{1}{2}$  annually, p. 427. In London 1 in  $20\frac{1}{2}$ , *ibid.* At Northampton 1 in  $26\frac{3}{4}$  die annually, *ibid.* And in the island of Madeira, for eight years, 1 in 50 died annually, *ibid.* A 19th part of the inhabitants of Stockholm die every year; and in the whole kingdom of Sweden, towns and country together, not more than a 35th part, p. 427. *n.* Average of deaths for ten years at Vaud, in Switzerland, 1 in 45 annually, p. 428. In Ackworth, in the county of York, for ten years to 1757, a 56th part had died annually, and in the year 1767 the annual average was a 47th part, *ibid.* Observations on this striking difference between the rate of human mortality in towns and in country parishes and villages, p. 428—434. Surveys in the year 1773 of the inhabitants at Hale, Horwich, Darwen, Cockey Moor, and Chowbent, in Lancashire, with the state of their births and deaths, p. 431, 432, *n.* Number of males and females born in different places, and at different periods, p. 435. The proportion of male to female births 20 to 19, *ibid.* Number of males living less than females, p. 435, 436. Males more short-lived than females, p. 436. Instanced in the parish of St. Sulpice, at Paris, during 30 years; and in Stockholm and the whole kingdom of Sweden during 9 years; *ibid.* Sweden gains above 20,000 inhabitants yearly by the excess of births above deaths, p. 437. The order of human mortality in Sweden, p. 438. Observations thereon, p. 439. Its order at Stockholm, p. 440. Observations thereon, p. 441. State of human mortality in the whole kingdom of Sweden and in its capital for 9 years, p. 442. Christenings and burials in the parish of Ackworth, in the county of York, for 20 years, p. 443, 444. Account of the inhabitants of Rome for 11 years, p. 445.
- Lightning*, an account of its effects on a house, which was furnished with a pointed conductor, at Tenterden, in Kent, p. 336.

- Wind*, Dr. his description and use of a portable wind gage, p. 353. See *Wind gage*.
- Loco-motion*, the power of, in certain animals, on what it depends, p. 214.
- London*, an abridged state of the weather there in the year 1774, collected from the meteorological journal of the Royal Society, p. 167. See *Weather*.—London and Edinburgh. a comparison of the heat of, p. 459. See *Heat*.—Average of deaths in London, p. 427.
- Longevity*, uncommon instances of, at Chester, p. 85.
- Lorimer*, Mr. J. his description of a new dipping-needle, p. 79. See *Dipping-needle*.
- Ludlam*, Rev. Mr. his astronomical observations made at Leicester, p. 366—372. See *Leicester*.
- Lyndon*, in Rutland, extract of a register of the barometer, thermometer, and rain, for the year 1774, kept there, p. 199.
- Lyons*, Mr. calculations in spherical trigonometry abridged, p. 470. See *Trigonometry spherical*.

## M.

- Machine* for raising water, an account of one at Oulton, in Cheshire, p. 277. See *Water*.
- Madeira*, island of, average of deaths there for eight years, p. 427.
- Magdalen islands*, sea-cow a native of, p. 249.
- Magnet*. See *Dipping needle*, and *Poles Magnetic*.
- Males* and females, number born at different places and at different periods, p. 333. 435. Male to female births 20 to 19, *ibid*. Greater number of males than females born, p. 335. Males living less than females, p. 335. 435, 436. More short-lived than females, p. 436. Number of males to females in different places, p. 334. More females than males baptized at St. Stephen's, Bristol, from 1754 to 1774, p. 332. Mr. Derham's calculation confirmed, p. 333. 435. Comparative view of the number of widowers and widows in different places, p. 335.
- Manchester*, and other adjacent places, observations on the state of population in, p. 322. An enumeration of the parish of Manchester, *ibid*. Number of inhabitants in the town, township, and parish of Manchester, p. 323. Striking disparity in the healthiness of the town of Manchester, and of the country round it, in the year 1772, *ibid*. To what attributed, p. 324. Proportion of annual deaths in Manchester and the adjacent country, p. 425.
- Marble*, found among Scaglia, p. 35.
- Marks* in children, the supposition that they depend upon the imagination and longing of the mother, entirely void of foundation, p. 317—320.
- Marriages*, proportion of to deaths, in different places and at different periods, p. 328, 329. See *Deaths*.
- Martins and Swallows*, an account of their torpidity, p. 343. See *Torpidity*.
- Mathew*, Rev. Mr. his proposal for measuring the attraction of some hill in this Kingdom by astronomical observations, p. 495. See *Attraction*.—His account of observations

observations made on the mountain Schehallien for finding its attraction, p. 500.  
See *Schehallien*.

*Measure* and weight, what was the ancient English, prior to the reign of Henry the Seventh, an inquiry to shew, p. 48. See *Weight and Measure*.

*Mucum*, universally found\* in the bowels of new-born children, nothing more than the *mucus* naturally secreted by the intestinal glands, p. 314, *n*.

*Meteorological* journal for the year 1774, kept at the Royal Society's house, p. 139. See *Tables*. — One kept at Bristol for the year 1774, p. 194. Why no important information has been derived from keeping meteorological journals, *ibid*. Tables given as an example of a method to remedy the neglect, p. 167—193.

*Middleton*, near Manchester, an account of births, deaths, and marriages, in the parish of, from 1663 to 1672, and from 1763 to 1772, p. 326, 327. Observations on the change in the proportion of births to deaths, and also to the marriages, at Middleton, during the last century, p. 327, 328.

*Monstrosity* and marks in children, do not depend upon the imagination and longing of the mother, p. 317—320.

*Monte Rosso*, a basaltine hill in the Venetian State, in Italy, p. 36. See *Rosso*.

*Monte Castello*,

*Monte Nuovo*,

*Monte Uliveto*,

} three of the Euganean basaltine hills, described, p. 38—41.

*Moon's* influence on the weather, table for trial of, p. 177, &c.

*Moon*, doubts concerning her supposed influence on the weather, p. 178—198. Conjectures concerning the origin of this general persuasion, p. 180—183. The appearances of the moon tokens of the weather but not causes, p. 180—182. Tables for trials of the moon's influence, p. 177. 193. 197. The Doctrine of the moon's influence had not gained ground among practical writers in the time of Aratus, p. 186.

*Mootegil*, natural ice seldom seen there, p. 255.

*Mortality*, human, observations on the difference between the duration of it, in towns and in country parishes and villages, p. 424. 428—434. Difference in the state of mortality in towns and in country parishes, whence it arises, p. 428. The apparent rate of mortality in towns, not magnified by an influx of people from the country, p. 428.

*Moscow*. Comparative heat of Moscow and Edinburgh, p. 460.

*Mountains*. See *Attraction*.

*Mourn-Hills* in Ireland composed of Granite and Lava, p. 34.

*Musical* instrument, an account of one brought from the Isle of Amsterdam in the South Seas, p. 67. Experiments thereon, *ibid*. Explanation of it according to its exact size, p. 69. The manner of blowing, and what tones produced, p. 69, 70. The tones adapted to English *concert pitch*, p. 70. The systems furnished correspond with the diatonic and chromatic genera, p. 71. Not capable of performing according to the enharmonic division of the tetrachord, *ibid*.

*Myrrh*, observations upon, made in Abyssinia, in the year 1771, p. 408. Myrrh of the moderns nothing different from that of the ancients, *ibid.* From whence it is brought, *ibid.* Troglodyte myrrh preferred to that of Arabia, p. 409. Why modern myrrh is generally worse in quality than the myrrh of the ancients, *ibid.* Why in some years this myrrh may not exist, *ibid.* The myrrh of the Troglodytes not all of equal goodness, p. 410. The cause of this difference, and how the best sort is procured, p. 410, 411. How new myrrh gets an oily covering, p. 412. A specimen of what the Troglodyte myrrh was, p. 412, 413. How to know Arabian myrrh from Abyssinian, p. 417.

## N.

*Nests*, where the sand-martins build theirs, p. 273.

*Noth*, Dr. his description of an apparatus for impregnating water with fixed air, p. 59.

See *Water*.

*Northampton*, average of deaths there, p. 427.

## O.

*Oats*, experiments on their power of generating heat, p. 451.

*Occultations* of  $\gamma$  and  $\alpha$  Tauri, at Leicester, p. 370. From Mr. Wargentia, p. 281.

*Oil*, causes hot water to boil briskly, p. 492.

*Opocalspum*, conjectures concerning, p. 413—416.

*Otiabite*, observations on the nose flute of, p. 72. See *Flute*.

*Outon*, in Cheshire, an account of a machine for raising water there, p. 277. See *Machine*.

## P.

*Papilion de Montagna*. See *Sand-martin*.

*Pendle-hill* and *Penny-gant*, their height, p. 498.

*Penny-weight*. See *Weight* and *Measures*.

*Peritival*, Dr. Thomas, his observations on the state of population in Manchester and other adjacent places, concluded, p. 322. See *Manchester*.

*Petrarch*, canon of the cathedral at Padua, relicks of him near Arquà, p. 35.

*Phosphoric spar*, mineral acid air produced from, p. 394.

*Pliny*, his eight critical days of the moon's age, p. 189—190.

*Poles*, magnetic, how to discover the poles of a spherical loadstone, p. 79. Generally found upon two points diametrically opposite to one another, *ibid.* The magnetic poles of the earth seem obliquely situated to one another, *ibid.* Where situated unknown, *ibid.* Accurate observations with a good dipping-needle, as near these magnetic poles as possible, the surest way to complete the magnetic theory of this globe,

p. 79, 80.

*De Polygoni* area vel perimetro maximis et minimis, inscriptis circulo, vel circumferentibus, p. 301. Vide *Theorema* I. II. III. IIII. V. VI. Theorems for the solution of polygons, p. 281, 282.

*Polypi*. Remarks on their reproduction and propagation, p. 231.

*Population*, observations on the state of, in Manchester, and other adjacent places, p. 322.

See *Manchester*.

*Pound*. See *Weights and Measures*.

*Præsepe et Ajelli*. See *Cancer*.

*Price*, Rev. Dr. his observations on the difference between the duration of human life in towns and in country parishes and villages, p. 424. See *Human life*.

*Priestley*, Rev. Dr. his account of further discoveries in air, p. 384. See *Air*.

PROBLEMS, *mathematical*.

If the two legs of a spherical triangle right-angled are given, to find the hypotenuse, one leg being small in comparison of the other, p. 475.

Having the hypotenuse and one of the angles, to find the base, p. 476.

Supposing the same things known as in the last, to find the perpendicular, when the hypotenuse is nearly a quadrant, p. 477.

Supposing a spherical triangle, in which are given two sides, with the included angle, to find the third side, p. 478.

Supposing the same things given, to find either of the angles, p. 482.

*Physical*, To find the time of descent in a circular arc, p. 287.

*Prognostication*, science of in its earliest state, p. 182. How corrupted, *ibid*.

*Prognostics* derived from the Sun, most of all to be relied on according to the ancients, p. 182. Prognostics derived from fixed stars, *ibid*.

*Propagation*, singular propagation of some species of sea anemonies, p. 222—230.

*Puy*, in Velay. St. Michael's church there built on a rock of *Lava brecciata*, p. 39.

## R..

*Rain*, a regular account of what quantity fell in London during the year 1774, p. 140.

164. An abridged view of it, p. 168.—At Lyndon, in Rutland, p. 199, 200.—

At Bristol, p. 194.

*Rameaux*, a basaltine hill in Velay, p. 13. See *Basalties*.

*Reproduction and propagation* of certain animals, p. 230—235.

*Richmond*, in Yorkshire, an account of, p. 329, 330. Births, deaths, and marriages there, for nine years, p. 329.

*Roebuck*, Dr. his comparison of the heat of London and Edinburgh, p. 459. See *Heat*.

*Rome*, average of deaths there for ten years, p. 329. Account of its inhabitants for 11 years, p. 445.

*Room*, heated, Experiments and observations in one, p. 111—123. 463—469. 484—



*Roffo*. Il monte Roffo, a basaltine hill in the Venetian State, p. 6. A topographical view of it. TAB. I. See *Basalties*. Singularities of the columns of Monte Roffo, p. 7. 8. 10, 11. The mountain formed of an inferior kind of Granite, which the columns resemble, p. 8. No single columns or loose fragments observed about Monte Roffo, p. 22. The Hill terminated by a regular convex summit, which forms a solid mass, p. 22. Of the physical topography and Vulcanic Productions of Monte Roffo, p. 36—38.

## S.

*St. Flour*, a city in Upper Auvergne, built on a giant's causeway, p. 15. Singularities of its basaltine columns, *ibid.*

*St. Michael's* church, at Puy, in Velay, built on a rock of lava, p. 39.

*San Bialfo*, in the Euganean hills, in Italy, a group of angular columns newly discovered there, p. 418. See *Basalties*. The rocks of San Bialfo abound with ferrugineous vitrifications, p. 422.

*San Giovanni*. See *Diavolo*.

*San Gotbard*, account of its parallel strata of Granite, p. 422.

*Sand-Martin*, or Bank-Martin, an account of the, p. 472. The least of the British *hirundines*, and the smallest known *hirundo*, *ibid.* They disclaim all domestic attachments, and delight in wild places and large waters, p. 272, 273. 275, 276. Built their nests in the bottom of deep holes in the sand or earth, p. 273. Make good dispatch in scooping these holes, p. 273, 274. Leave several holes unfinished, and conjectures concerning them, p. 274. After some years, they forsake their old holes, and bore new ones, *ibid.* Much annoyed by fleas, *ibid.* Sometimes dispossessed of their breeding-holes by the house-sparrow, p. 275. These caverns not made use of by way of *hybernacula*, *ibid.* They arrive about the same time with the swallow, and lay from four to six eggs, *ibid.* Bring forth their young rather sooner than the swallows, *ibid.* These *hirundines* no songsters, *ibid.* Breed twice, and withdraw about Michaelmas, p. 276. Much the rarest species in the south of England, *ibid.* Peculiar manner of flying, *ibid.* The flight of all *hirundines* influenced by, and adapted to, the peculiar sort of insects which furnish their food, *ibid.* Sand-Martin differ from their congeners in diminutiveness and colour, *ibid.* Sold in Spain for the table, and called *papilion de montagna*. See *House-Swallow*, *Swift*.

*Sasse*, p. 414.

*Saurin's* ring, observations on its second disappearance and re-appearance again, p. 294, 295.

*Scaglia*, or *Scagliola*, p. 34.

*Scheiballen*, the mountain, an account of observations made on, for finding its attraction, p. 500. Every thing necessary for carrying the design into execution prepared, p. 500, 501. A tour made to find the best hill for the experiment, p. 502, 503. This mountain

mountain chosen for the purpose, p. 503. Expence attending it too great for the common funds of the society, *ibid.* This difficulty removed by his Majesty's bounty, p. 503, 504. Mr. Maitland requested to carry the experiment into execution, p. 504, 505. An account of the observations requisite to be formed, p. 505—508. The observatory fixed on the south side of Seehallien, *ibid.* Preparations for making the observations, p. 508, 509. Seventy-six observations taken on 34 stars, with the plane of the sector turned to the east, p. 509. Ninety-three observations taken on 39 stars, with the plane of the instrument facing the west, *ibid.* Method of verifying the position of the sector, p. 509—511. Two meridian lines obtained by observations taken with the quadrant and transit instrument, p. 511. Methods taken to determine the figure and dimensions of the hill, p. 512—516. The observatory and instruments removed to the north side of the hill, p. 516. Sector put up with its plane facing the west, p. 517. Instrument brought into the meridian by the transits of the stars, p. 517, 518. Sixty-eight observations taken on 32 stars, the plane of the sector being turned to the west, p. 518. One hundred observations taken on 37 stars, with the plane of the instrument facing the east, *ibid.* Forty-three different stars observed in all, on both sides the hill, and 337 observations taken, p. 519. Remarks on the foregoing observations, p. 519—521. In what manner the two bases were measured, p. 521—523. Their mensuration, p. 523. The experiment honoured with the presence of many gentlemen conversant in mathematical and philosophical learning, p. 524, 525. The use of the two theodolites at once in completing the survey of the hill, p. 525. The result of computations made upon those operations for deducing the effect of the attraction of the mountain, p. 526—532. Consequences flowing from this experiment, tending to illustrate some important questions in natural philosophy, p. 532—534. Observations of stars made with the zenith sector on the mountain Seehallien, for discovering its attraction, p. 536.

*Scotch fir*, has a power of generating heat, p. 451, 452.

*Sea*, scarce any perceivable difference of heat at different time of the day in the open sea, p. 204.

*Sea Anemonies*, a second essay on their natural history, p. 207. A fourth and fifth species of that animal, *ibid.* Ideas suggested by the last experiments, p. 208—211. This animal exceedingly gelatinous, p. 209, 210; and irritable to such a degree as to be affected by light, p. 209. Observations and experiments continued, p. 211. Inferior half of the first species daily recovering strength above a twelvemonth after it had been cut, *ibid.* Its water infected by accident, causing it to throw up a great quantity of its intestines, p. 212. Considerably shrunk, *ibid.* A second discharge of its intestines by the water being again infected, *ibid.* Totally dissolved, *ibid.* Observations on the foregoing experiment, p. 212, 213. Upper part of another sea-anemony, of which the inferior was become a perfect animal, lived six months, p. 213. Some cut diametrically and perpendicularly, not essentially hurt by the operation, p. 213, 214. These semi-anemonies appear perfect animals, and per-

form

- form all their functions, p. 214. Reflection concerning these animals, *ibid.* Further observations on them, p. 215. Conjecture concerning their propagation, p. 216. The second species difficult to preserve, p. 217. Safe method of taking them, *ibid.* They make great havock in the sea, *ibid.* Their limbs being clipped, new perfect ones imperceptibly shoot out, p. 217, 218. Observations and experiments on the second species, p. 218, 219. This kind of sea-anemonies preferable for the table, 219. The semi-anemonies of the third species entirely recovered the parts they had been deprived of, p. 220. Several of these animals, cut through the middle by accident while in the sea, soon recovered, p. 221. Conjecture concerning the generation of this species, p. 221, 222. Observations and experiments on the fourth species of sea-anemonies, p. 222—229. Remarks on it, p. 229, 230. A monster of this kind, p. 223, 226, 229, 235, 236. How produced, its internal structure an interesting object, *ibid.* Its mouth and limbs described, p. 224. Conjectures concerning the generation of this species, p. 225—227. How these animals are produced, p. 227—229. Experiments thereof, p. 230, 231. Fresh-water polypi increase by section, p. 231, 232. Remarks concerning re-production, p. 232—235. The sections of this species of sea-anemonies like the former, except that the upper half never produced a new basis, p. 235. Description of a fifth species of sea-anemonies, p. 236—238. An apology for not enlarging on the performance of their different functions, p. 238, 239. Sea-anemonies recommended to form a sea-barometer, p. 240, 245, 246. What species the fifth, and how to be managed, p. 240—244. What weather may be expected from the different state of the anemonies, p. 241, 242. Other animals influenced by the approaching changes of weather, p. 246. Anemonies might furnish ornaments in naval and hydraulic architecture, p. 246, 247. Explanation of Tab. VI. representing a group of sea-anemonies of the fourth species, 248.
- Sea-Cow*, an account of the, and the use made of it, p. 249. A native of the Magdalen Islands, *ibid.* Great numbers remain on shore for several days in fair weather, *ibid.* They retreat to the water on the appearance of rain, *ibid.* Large and unweildy, *ibid.* Their time of gestation, p. 249. Account of their landing, p. 250. Method of separating them, to be killed at leisure, p. 250, 251. What use is made of their skin and teeth, p. 251.
- Shuidhani*, Mr. his account of the sea-cow, and the use made of it, p. 249. See *Sea-Cow*.
- Skidda and Helwellin*. Height of these hills above the intermediate valley, p. 498.
- Small pox*, remarks on, p. 87.
- Snow*, experiments on its power of resisting external cold, p. 450.
- Snow*, why it melts faster on grass than on gravel, p. 122. Snow at London in the year 1774, p. 73.
- Songster*, the swallow a delicate one, p. 262.
- Spar*, phosphoric, a new mineral acid produced from it, p. 394.
- Spherical Trigonometry*, calculation in, abridged, p. 470. Method of solving several of the most useful questions in spherical trigonometry, somewhat similar to that used in approxi-

- approximating to the roots of algebraic equations, p. 472—474. Problems exemplifying the practical application of this method, p. 475—483.
- Springs*, near Edinburgh and at London, mean heat of, 460, 461. Connexion between the mean heat of a country and the mean heat of its springs, p. 461.
- Staffa*, a western island of Scotland, volcanic phenomena lately discovered there, p. 10.
- Stacte*, what it was, p. 411, 412.
- Stedman*, Dr. on triangles described in circles and about them, p. 296. See *Triangles*.
- Stork*, Joshua, Esq. his account of a musical instrument brought from the isle of Amsterdam, in the South Seas, p. 67. See *Musical instrument*.—His remarks on a larger system of reed pipes from thence, with observations on the nose flute of Otahoeite, p. 72. See *Flute*.
- Stockholm*, average of deaths for these six years, p. 426, 427, *n*. Its order of human mortality, p. 440.
- Strange*, Mr. his account of two giants causeways, and other curious volcanic concretions, in the Venetian State, in Italy, p. 5.—His account of a curious giant's causeway, or group of angular columns, newly discovered in the Euganean hills, near Padua, in Italy, p. 418. See *Basiltes*, *Rosso*, *Diavolo*, *Euganean hills*, *Vulcanic hills*, *Granite*, *Granitellos*, *Giants-causeway*, *Lava*, *San Biagio*, *San Gotthard*.
- Surinam*, the gymnotus electricus brought from thence, p. 102, 109.
- Sutton Coldfield*, in Warwickshire, an account of, with an extract from the parish register, p. 331.
- Swallows*, House, an account of the, p. 258. The bird's want of sagacity, p. 263. See *House Swallow*.—Swallows and Martins, an account of their torpidity, p. 343. See *Torpidity*.
- Sweden*, average of deaths in, p. 427, *n*. Gains above 20,000 inhabitants yearly by the excess of births above deaths, p. 437. The order of human mortality there, p. 438; for 9 years, p. 442.
- Swift*, or Black Martin, account of the, p. 264. Largest of the British *hirundines*, *ibid*. Latest comer, *ibid*. Their nests defective in architecture, *ibid*. Suspected of expelling the house-sparrows from their nests, and taking them to their use, 264, 265. Where they build, *ibid*. Begin nesting about the middle of May, *ibid*. Copulate on the wing, 263, 266. Common for insects to do so, 266. The Swift appears to live more in the air than any other bird, *ibid*. Lays invariably but *two eggs* at a time, *ibid*. A most alert bird, rising early and retiring late, *ibid*. Assemble in the air in groups, just before they retire, *ibid*. Fond of sultry, thundery weather, 267. In hot mornings, the mates form little parties, and serenade their sitting hens, *ibid*. The hen relieves herself from her duty of incubation, for a few minutes only, just at dark, *ibid*. Swifts while they have young have a little lump of insects pouched under their tongues, *ibid*. Feed in general in a higher district than the other species, *ibid*. Range to a great distance, *ibid*. Their wings longer in proportion than almost any other bird, *ibid*. When they mate in flight, their wings meet over their heads, *ibid*. Descend below their

their usual range at certain times in summer, hovering over pools and streams for hours together, in search for flies, p. 267, 268. Account of their young, p. 268. Swifts sometimes pursue and strike at hawks, *ibid.* Regardless of rain, unless the showers are heavy, attended with much wind, p. 268, 269. Conjecture from a circumstance respecting their *colour*, that they retire to rest for a season, p. 269. Breed but *once* in a summer, *ibid.* Withdraw soon after the flight of their young, *ibid.* Other *hirundines* increase five times as fast as the Swift, p. 369, 370. Singular in their retreat, the main body retiring early in August, 270. Remarks thereon, *ibid.* These *hirundines* never congregate with their congeners, *ibid.* Fearless while haunting their resting places, *ibid.* Much infected with the *hippoboscæ hirundinis*, *ibid.* Swifts no songsters, *ibid.* Never settle on the ground but by accident, their shortness of legs and length of wings preventing their rising, 271. Other peculiarities attending them, *ibid.* Frequent the Tower, and some of the churches of the Borough next the fields, not venturing to the close patts of London, *ibid.* Called by the Swedes *ring svala*, 272. See *Houfe-Swallow*, *Sand-Martin*.

## T.

## TABLES.

Deaths, ages, and conditions, in Chester, for the year 1773, p. 88.

Diseases of different ages in Chester, for ditto, p. 89.

Diseases of different months in Chester, for ditto, p. 90.

Of marriages, baptisms, and burials, in Chester, for ditto, *ibid.*

Meteorological Journals for the year 1774, kept at the Royal Society's house.

For Jan. p. 140, 141. Feb. p. 142, 143. March, p. 144, 145. April, 146, 147. May, p. 148, 149. June, 150, 151. July, p. 152, 153. Aug. p. 154, 155. Sept. p. 156, 157. Oct. p. 158, 159. Nov. p. 160, 161. Dec. p. 162, 163. Greatest, least, and mean height, of the Thermometer without and within, and of the Barometer in each month throughout the year, p. 164.

The quantity of rain in the whole year, *ibid.* The variation of the magnetic needle, p. 165.

An abridged view of the winds at London in the year 1774, compiled from the Meteorological Journal of the Royal Society, p. 168. Sub-division of the S. W. wind, p. 170. Sub-division of the N. E. wind, *ibid.* Sub-division of the S. E. wind, p. 171. Sub-division of the N. W. Wind, *ibid.*

General state of the winds collected from the preceding tables, p. 172.

Number of fair and frosty days in each half month and in the whole year, p. 173.

For trial of the influence of the winds upon the barometer, p. 175.

For trial of the moon's influence, p. 177.

Changes of weather in particular days, p. 193. Explained, p. 197.

Meteorological journal for the year 1744, kept at Bristol, state of barometer, and quantity of rain, in each month throughout the year, p. 124. Winds and frosty days ditto, p. 195. For trial of the moon's influence ditto, p. 197.

Extracts

Extracts of a register of the barometer, thermometer, and rain, at Lyndon, in Rutland, for the year 1774, p. 199. Wettest months from one to twelve, p. 200.

Heights of the thermometer in each month of the year 1767, at Allahabad in the East Indies, p. 202.

Going of Mr. Wollaston's clock, from Dec. 1773 to Jan. 1775, p. 293.

State of Mr. Wollaston's thermometer, barometer, and hygrometer, during nine months of the year 1774, p. 294.

Astronomical observations for determining the latitude of Leicester, made in the year 1774, p. 366. Zenith distance of  $\beta$  Draconis and  $\gamma$  Draconis, *ibid.* Zenith distances of  $\alpha$  Herculis with the state of the barometer and thermometer, p. 368. Zenith distances of the pole-star, p. 369. Occultations of  $\gamma$  and  $\alpha$  Tauri, p. 370. Observations on the transits of the sun, serving to examine the Clock, p. 371. Zenith distances taken to ascertain the absolute error of the clock, *ibid.*

Proportion of births to marriages in different places, and at different periods of time, p. 328, 329.

Comparative view of the number of males and females baptized in different places, p. 333.

Comparative view of the number of males and females in different places, p. 334.

Comparative view of the number of widowers and widows in different places, p. 335.

Number of males and females born in different places, and at different periods, p. 435.

The order of human mortality in Sweden, p. 438.

The order of human mortality at Stockholm, p. 440.

State of human mortality in the whole kingdom of Sweden and in its capital, for nine years, p. 442.

Christenings and burials in the parish of Ackworth, in the county of York, from March 25, 1747, to March 25, 1757, p. 443.

Christenings and burials in the same parish, from March 25, 1757, to March 25, 1767, p. 444.

Account of the inhabitants of Rome, from 1762 to 1771 p. 445.

Mean heat in Pall Mall, London, during the years 1772, 1773, 1774, p. 461.

Mean heat at Hawkhill, situated about one mile north of Edinburgh, and 103 feet above the level of the sea, during the years 1772, 1773, 1774, p. 462.

Observations of stars made with the zenith sector on the mountain Schiehallien for discovering its attraction, p. 536.

*Tenderden*, in Kent, an account of the effects of lightning on a house there, which was furnished with a pointed conductor, p. 336. See *Lightning*.

*Teneriff*, Pic of, conjectures concerning, p. 30.

THEOREMS, *mathematical*.

*Theorema* I. Si linea recta arcum circulem contingentibus duabus interceptum contingat, segmentum ejus, contingentibus primo positis interceptum, in contactus sui puncto vel æqualiter vel inæqualiter divisum est, prout arcus ipse circularis æqualiter vel inæqualiter in eodem puncto divisus est. Segmentaque arcus (inæqualiter scilicet divisi) et rectæ contingentis majora et minora ab iisdem sunt partibus mutui contactus, p. 301, 302.

II. Linea recta quæ arcum circulem contingentibus duabus interceptum in puncto medio contingit, & contingentibus primo positis hinc inde occurrit, minima est omnium quæ, eundem arcum contingentes, contingentibus primo positis intercipiuntur, p. 303—306.

III. Polygonorum omnium, lateribus numero datis, datum circulum circumscribentium, æquiangulum perimetro minimum est, p. 306—308.

IV. Polygonorum omnium, lateribus numero datis, datum circulum circumscribentium, æquiangulum areâ minimum est, p. 308.

V. Polygonorum omnium, lateribus numero datis, dato circulo inscriptorum, æquilaterum perimetro maximum est, p. 308.

VI. Polygonorum omnium, lateribus numero datis, dato circulo inscriptorum, æquilaterum areâ maximum est, p. 310.

*Theorems*. Two for the solution of polygons, p. 281, 282.

Investigation of a general one for finding the length of any arc of any conic hyperbola, by means of two elliptic arcs, p. 283—286. Some other new and useful theorems deduced therefrom, p. 286—289.

An equilateral triangle inscribed within a circle, larger than any other triangle that can be inscribed within the same circle, p. 296—298.

An equilateral triangle described about a circle, less than any other triangle that can be described about the same circle, p. 298.

The square of the side of an equilateral triangle inscribed in a circle, equal to a rectangle under the diameter of the circle, and a perpendicular let fall from any angle of the triangle upon the opposite side, p. 299, 300.

*Theophrastus*, an assessor of the moon's influence upon the weather, p. 189.

*Thermometer*, state of it at London throughout the year 1774, p. 140—164.—At Lyndon, in Rutland, p. 199.—At Chislehurst, in Kent, p. 294.

*Thermometer*, kept within doors, a very indifferent measure of the heat of any climate, p. 202. The least and greatest heights of the thermometer at Allahabad in each month, p. 203. State of the thermometer between Mdrass and the Southern Tropic, p. 204. Its state from thence to about 15° E. of the Cape of Good Hope; to St. Helena; to lat.

- lat.  $43^{\circ} 14' N$ ; and to the British Channel, *ibid.* State of the thermometer in the soldiers barracks at Allahabad, *ibid.*
- Thermometrical* observations, an account of some made at Allahabad, in the East Indies, in lat.  $25^{\circ} 30' N$ . during the year 1767, and also during a voyage from Madras to England in the year 1771, p. 203. See *Allahabad*.
- Thunder* at London in the year 1774, p. 174.
- Toad*, experiments on its power of resisting external cold, p. 450.
- Torpedo*, experiments on, made at Leghorn, p. 1. Its effect the same, whether the fish be in or out of water, p. 1—3. Said to have very little force in winter, and cannot live long out of water, p. 2. Other fishes not affected by their company, *ibid.* Pressing gently the two soft bodies at the side of the head, called *miskuli sakati*, produced the same sensation as if a great number of very small electrical bottles were discharged through the hand in quick succession, *ibid.* These shocks stronger at last than in the beginning, p. 2. Its powers not altered by different degrees of pressure, p. 3. When suspended by a ribbon, light bodies near it not attracted, *ibid.* More frequent shocks caused by the fish bending its body, p. 4. An account of its dissection, *ibid.*
- Torpidity* of swallows and martins, an account of the, p. 343. Appear on the banks of the river Dart in the beginning of November, *ibid.* Brought from their winter quarters by a fine, warm afternoon, 344. As the sun lowered, they all returned into the fissures of the rocks, *ibid.* Not a swallow amongst them, *ibid.* Vast flights of swallows go towards the South every autumn, *ibid.* The season being uncommonly cold, the martins remain in the rocks, in a torpid state, at the end of March, p. 345. Dormice and bats frequently found in a state of torpidity, p. 346. Martins seen in Totness in Dec. and Jan. *ibid.* Three black martins or swifts found in an old oak during the winter, p. 347. They recovered strength enough to fly, on being laid before a fire, *ibid.* The objection brought against the opinion that those birds remain torpid during the winter, *ibid.* Answered, p. 347, 348. Several proofs of the torpidity of birds, p. 348—350. The opinion that some of the same species of birds do emigrate, and others remain torpid during the winter, when discredited, p. 350. The *viscera* of several torpid bats examined, p. 351. Observations thereon, *ibid.* Torpidity of animals in the winter accounted for, p. 453.
- Towns*, great, in a peculiar degree fatal to children, 324—326.
- Triangles* described in circles and about them, p. 296. An equilateral triangle inscribed within a circle, larger than any other triangle that can be inscribed within the same circle, p. 296—298. An equilateral triangle described about a circle, less than any other triangle that can be described about the same circle, p. 298. The square of the side of an equilateral triangle inscribed in a circle, equal to a rectangle under the diameter of the circle; and a perpendicular let fall from any angle of the triangle upon the opposite side, p. 299, 300.
- Froglodyria*, its ancient limits, p. 408.



*Trigonometry*, spherical calculations in it abridged, p. 470.

*Tulips*, roots of, have a power of generating heat, 451.

## U.

*Upper Auvergne*, in France, a city and town there built on giants causeways, p. 15; 16.

## V.

*Vandelli*, figure of the Buccinum in his treatise *De Thermis Patavinis*, inaccurate, p. 46.

*Varro*, his childish rule for predicting the weather, p. 183.

*Vaud*, in Switzerland, average of deaths there for ten years, 428.

*Vegetables*, experiments on, with regard to their power of producing heat, p. 450—458. Must be deprived of the principle of vegetation, before they can be frozen, p. 454. Their internal temperature susceptible of great variations, *ibid.* Their power of producing heat, on what it depends, 454. 457. By what principles they are adapted to different climates, p. 455. Texture of vegetables much changed by loss of life, *ibid.* Roots capable of resisting cold more than the stems, *ibid.* In what vegetables and animals differ, p. 456.

*Velay*, in France, basaltine hills there, p. 9. 11. 12. 14. 21. 22. A church built on a rock of lava, p. 39. Basaltine hills of Velay mostly terminated by regular convex summits, which form a solid mass, p. 22.

*Venda* mount, height of, p. 34.

*Venetian* State, two giants causeways, and other curious vulcanic concretions, discovered there, p. 5. See *Basalties*, *Rosso*, *Diavolo*.

*Vesuvius*, the inside of the funnel preserves vestiges of its primary organization, p. 28. Conjecture concerning an original mountain under its Lava, *ibid.* The whole mountain not produced by successive eruptions, p. 29.

*Vicentine* and *Veronese*, vulcanic mountains, p. 24. 26. More frequent in the Vicentine than the Veronese district, p. 44.

*Vulcanic* phenomena, abound in the Northern part of Italy, p. 42. See *Hills Vulcanic*.

*Volcanos* of the Andes, conjectures concerning, p. 30. Abound in fossil Glass, p. 40.

## W.

*Wales*, mountain of, supposed to contain prismatic basaltine columns, p. 16.

*Wargentin*, Mr. his observations of the occultations of  $\alpha$  and  $\gamma$  Tauri, p. 181.

*Water*, impregnated with fixed air, description of an apparatus for, p. 59. Operations on Dr. Priestley's apparatus for this use p. 59, 60. Part of it offensive, p. 60.

Dr.

Dr. Nooth's apparatus described, p. 61. Manner of conducting the process, and ingredients necessary, p. 61—63. Process repeated four or five times with the same water before it is highly impregnated, p. 64. This apparatus very useful in chemistry and pharmacy, p. 65. Glass valve preferable to an ivory one in the apparatus, p. 65, 66. — Water, the supposed effect of boiling upon, in disposing it to freeze more readily, ascertained, p. 124. Experiments relative to the freezing of boiled and unboiled water, *ibid.* Ice appeared first on the boiled water, *ibid.* Unboiled water by stirring freezes as soon as the boiled, p. 125. The congelation, though later, more powerful in the unboiled, *ibid.* Observations thereon, p. 126—128. Indians boil their water, to obtain ice, p. 126, 253. Water may be cooled below the freezing point without freezing, p. 128.—Water, an account of a machine for raising, at Oulton, in Cheshire, p. 277. Mode of raising water by its momentum new, *ibid.* Plan and description of the work, p. 277—279.

*Weather*, an abridged state of the, at London, in the year 1774, collected from the meteorological journal of the Royal Society, p. 167. An abridged view of the winds and rain at London in the year 1774, with an explanation of the table, p. 168. What winds prevailed most in the year, p. 169. Observations on the quantity of rain which fell in each month, *ibid.* Rule observed in collecting the rain of the several months, p. 169, 170. Sub-division of the S. W. and N. E. wind, with an explanation of the tables, p. 170, 171. Observations thereon, p. 171. Sub-division of the S. E. and N. W. wind, *ibid.* Observations thereon, p. 171, 172. A check upon the work of making the tables, p. 172. A general state of the winds, according to their different degrees of prevalence, *ibid.* The number of fair and frosty days in each half month, and in the whole year, p. 173. The number of snowy days in the year, *ibid.* What winds attended these particular days, p. 173, 174. Two thunder storms this year, p. 174. Height of the barometer and thermometer at each time, *ibid.* For trial of the influence of the winds upon the barometer, p. 175. Observations thereon, p. 176. For trial of the moon's influence, p. 177. Explanation of the table, p. 178. 189. 197. Popular persuasion of the moon's influence upon the changes of the weather, particular enquiry concerning it, p. 178—193.

*Weather*, change of, influence on animals, p. 246.

*Weight and measure*, what was the ancient English, prior to the reign of Henry the Seventh, an inquiry to show, p. 48. Sealed standards of weights and measures ordained by William the Conqueror and succeeding kings, p. 48, 49. The standard weight and measures, p. 48—51. Henry VIII. abolished the old pound, and established the Troy weight, p. 56. He first directed the use of the Averdupois weight, p. 57. Troy pound heavier than the old Tower pound, p. 49, 50, 56, 57. Troy pound the same as now used, p. 49. The difference of the weights clearly demonstrated by an experiment of the weight of a penny weight, raised from 32 grains of wheat, p. 49, 50. The wine and corn gallon the same, by the statutes of Henry III. and Edward I. p. 50. The wheat gallon larger than the wine gallon, by a statute of Henry VII. *ibid.*

He

He altered both weights and measures, p. 52. 56, 57. The weight and measure the same from William the Conqueror to Henry VII. p. 51. One weight, one measure, and one yard, to be used throughout the land, p. 52. In the reign of Henry VI. corn bought by the fath, *ibid.* The fath prohibited, p. 52. 57. Cubical contents both of the gallon and bushel measures, p. 53, 54. Statute of 12 Henry VII. which altered the ancient weights and measures, and some observations thereon, p. 54—58.

*Wheat.* Weight of grains of wheat, p. 49.

*Wernside,* its height, p. 498. The probable quantity of its attraction, *ibid.*

*White,* the Rev. Gilbert, his account of the house-swallow, swift, and sand-martin, p. 258. See *House-Swallow*, *Swift*, *Sand-Martin*.

*Whitcomb,* Mr. John, his account of a machine for raising water, at Oulton, in Cheshire, p. 277. See *Water*.

*Widowers* and widows, number of, in different places, p. 335.

*Widows* living, almost double the number of widowers, p. 335.

*Williamson,* Dr. Hugh, his experiments and observations on the gymnotus electricus, or electrical eel, p. 94. See *Gymnotus Electricus*.

*Wind Gage,* description and use of a portable one, p. 353, 354. Its use and management, p. 354—364.

*Winds,* state of them at London, during the year 1774, p. 140—164. An abridged view of it, p. 168—172. Winds, an abridged table of them at Bristol, for the year 1774, p. 195. Influence of winds upon the barometer, p. 175, 176.

*Wollaston,* the Rev. Francis, his observations on the clock, thermometer, barometer, and hygrometer, made at Chislehurst, in Kent, p. 290. Observations on the going of the clock, p. 290. 292. Differences of loss and gain when the clock was in some degree foul, p. 291. Differences of loss and gain after the clock was cleansed, *ibid.* July and August the months for greatest acceleration, *ibid.* January and February for retardation, *ibid.* The clock so fixed, as not easily to be affected by any tremulous motion, p. 291, 292. Going of the clock from December 1773 to January 1775, p. 293. State of the thermometer without doors and within, and of the barometer, and hygrometer, during nine months of the year 1774, p. 294. Observations on the second disappearance of Saturn's ring, and re-appearance of it again, p. 294, 295.

*Women* live longer than men, p. 84.

*Woulfe,* Mr. his experiments on a new colouring substance from the island of Amsterdam, in the South Seas, p. 91. See *Colouring substance*.

*Wounds,* conjectures concerning the consolidation of, p. 209.

The End of the Sixty-fifth VOLUME.

*The Number of Plates in this Volume (including the Musical Instrument in p. 68.) is FIFTEEN.*

# T H E L I S T

O F

## T H E R O Y A L S O C I E T Y.

MDCCLXXV.

---

His Sacred Majesty King GEORGE III. Patron.

His Sacr. Majesty STANISLAUS AUGUSTUS,  
King of POLAND.

His Sacr. Majesty CHRISTIAN King of DENMARK.

His most Serene Highness CHARLES Margrave of BADEN DOURLACH.

His most Serene Highness FRANCIS Duke of MODENA.

His most Serene Highness PETER Duke of COURLAND.

His most Serene Highness CHARLES WILLIAM FERDINAND,  
Hereditary Prince of BRUNSWICK.

---

### T H E P R E S E N T C O U N C I L.

---

Sir JOHN PRINGLE, Bt. PRESIDENT.	Matt. Maty, M.D. Sec. { <i>Ac. R. Ber. Stu.</i> <i>&amp; Harl. Soc</i>
Sir Joseph Ayloff, Bart.	Hon. Constantine John Phipps.
Joseph Banks, Esq; { <i>Ac. R. St. Soc.</i> <i>Ac. R. P. Cor.</i>	Sir James Porter, Knt.
John Bentinck, Esq;	Richard Price, D. D.
Sir James Burrow, Knt. V. P.	Charles Rogers, Esq;
Hon. Henry Cavendish, <i>Mus. Br. Cur.</i>	John Ross, D. D.
Samuel Horsley, LL. D. Sec.	William Roy, Esq;
Mr. John Hunter.	Dan. Ch. Solander, M.D. { <i>Ac. R. St. Soc.</i> <i>Ac. R. P. Cor.</i>
Mr. Timothy Lane.	William Watson, M.D. V. P. { <i>Mus.</i> <i>Br. Cur.</i>
Nevil Maskelyne, B.D. { <i>Astr. Reg.</i> <i>S. Gotl. Soc.</i>	Samuel Wegg, Esq; Treas. V. P.
	Mr. Peter Woulfe.

---

This COUNCIL is to be continued till SAINT ANDREW's Day, 1775.

## HOME MEMBERS.

Robert Adam, Esq;  
 Nathan Alcock, M. D.  
 John Anderson, A.M. { *Pb. N.*  
                                   *Pr. Gl.*  
 Thomas Anguish, Esq;  
 John Arbuthnot, Esq;  
 Shuckburgh Ashby, Esq;  
 Thomas Astle, Esq;  
 Alexander Aubert, Esq;  
 George Baker, M.D. *Med. Regim.*  
 Roger Baldwin, M. A.  
 Edward Bancroft, M. B.  
 Hon. Daines Barrington,  
 Edward Barry, M. D.

CHARLES MOSS, Lord Bishop of  
 BATH and WELLS.

Mr. David Bayford.  
 T. Butterworth Bayley, Esq;  
 Hon. Topham Beauclerk.  
 Mr. John Belchier.  
 Rich. H. Alex. Bennet, Esq;  
 John Blair, LL. D.  
 Charles Blagden, M. D.  
 Sir Francis Blake, Bart.  
 Richard Blyke, Esq;  
 Benjamin Booth, Esq;  
 R. Wilbraham Bootle, Esq;  
 Thomas Brand Hollis, Esq;  
 Gustavus Brander, Esq; { *Mus. Br.*  
                                   *Cur.*  
 Ow. Salusbury Brereton, Esq;  
 Richard Brocklesby, M. D.  
 Isaac Hawkins Browne, Esq;  
 William Brownrigg, M. D.  
 Mr. Patrick Brydone.

DAVID Earl of BUCHAN.

Charles Burney, *Mus. D.*  
 Peter Burrell, Esq;  
 Robert Burrow, Esq;  
 Amyas Bushe, Esq;

CHARLES Lord CADOGAN, *Mus. Br. Cur.*

Sir James Caldwell, Bart.

CHARLES Lord CAMDEN.

John Campbell, Esq;  
 Peter Canvane, M. D.

CHARLES Marquis of CARMAR-  
 THEN.

John Carnac, Esq;

Lord CH. CAVENDISH, *Mus. Br. Cur.*

John Caverhill, M. D.  
 Anthony Chamier, Esq;  
 Mr. John Chandler.

JAMES Earl of CHARLEMONT.

Charles Chauncey, M. D.

WILLIAM Earl of CHATHAM.

JOHN Earl of CLANRICARDE.

Samuel Clarke, Esq;  
 Mr. Josiah Colebrooke.  
 Charles Collignon, M. D.  
 James Comyn, Esq;  
 Mr. Benjamin Cook,  
 Sir J. Mordaunt Cope, Bart.  
 Robert Salusbury Cotton, Esq;  
 Mr. Jo. Lodge Cowley.  
 Patrick George Craufurd, Esq;  
 Lewis Crussius, D. D.  
 John Cuthbert, Esq;  
 Alexander Dalrymple, Esq;  
 Mr. James Dargent.

WILLIAM Earl of DARTMOUTH,  
                                   *[Mus. Brit. Cur.]*

John Darker, Esq;  
 Erasmus Darwin, M. D.  
 Solomon Dayrolle, Esq;  
 Edward Delaval, Esq;  
 Thomas Dickson, M. D.  
 Hon. Charles Dillon.  
 Baron Thomas Dimfdale.

Robert

Robert	Dingley, Esq;	Tho.	Gisborne, M.D. { <i>Med. Reg. ad Fam.</i>
Abraham	Dixon, Esq;	Samuel	Glaſſe, D. D.
William	Dixon, Esq;	Will. Mann	Godſchall, Esq;
Charles	Douglas, Esq;	Iſaac	Goffett, M. A.
Matthew	Duane, Esq; { <i>Myſ. Brit. Cur.</i>	William	Gould, M. A.
Andr. Coltee	Ducarel, LL. D.	Charles	Gray, Esq; <i>Myſ. Brit. Cur.</i>
Daniel	Dumareſque, D. D.	Richard	Green, D. D.
Thomas	Dummer, Esq;	John	Gregg, Esq;
Thomas	Dundas, Esq;	Hon. Charles	Grevile.
Francis	Duroure, Esq;	Mr. Richard	Grindal.
Will. Benſon	Earle, Esq;	Steddy	Grinfield, Esq;
Henry	Ellis, Esq;	HENEAGE	LORD GUERNSEY.
John	Ellis, Esq; <i>Ac. Reg. Succ. Soc.</i>	Hon. Charles	Hamilton.
John	Ellis, Esq;	Hugh	Hamilton, D. D.
Rt. Hon. Welbore	Ellis.	Hon. Sir W.	Hamilton, K. B.
Thomas	Emlyn, Esq;	Bartholomew	Hammond, Esq;
Mr. Robert	Erſkine.	SIMON	Earl HARCOURT.
BROWNLOW	Earl of EXETER.	Caleb	Hardinge, M. D. { <i>Med. R. Ext.</i>
Richard	Eyre, D. D.	PH. E. of	HARDWICKE, <i>Myſ. Br. Cur.</i>
William	Falconer, M. D.	Samuel	Harper, M. A.
William	Farr, M. D.	Mr. Daniel	Harris.
William	Fauquier, Esq;	James	Harris, Esq; { <i>Myſ. Brit. Cur.</i>
Samuel	Felton, Esq;	Mr. William	Harrifon.
Mr. James	Ferguson.	Edward	Haſted, Esq;
WASHINGTON	Earl FERRERS.	James	Hawley, M. D.
Keane	Fitzgerald, Esq;	Richard	Hazard, Esq;
RICHARD L. Viſc.	FITZWILLIAM.	Thomas	Healde, M.D. { <i>Med. Pr. Gr.</i>
Sir Martin	Folkes, Bart.	John	Heathcote, Esq;
Mr. John Reinhold	Forſter.	Sir Thomas	Heathcote, Bart.
Thomas	Forſter, M. A.	William	Heberden, M. D.
John	Fothergill, M. D.	Mr. William	Henley.
Thomas	Frankland, Esq;	William	Henry, D. D.
Ben. Franklin, LL.D.	{ <i>Ac. R. Par. Soc. Ext. &amp; S. Gott. Soc.</i>	WILLS	Earl of HILLSBOROUGH.
Naphtali	Franks, Esq;	Joſeph	Hoare, D. D.
Sir Charles	Frederick, Kt. of Bath.	Sir Henry	Hoghton, Bart.
John	Frere, Esq;	Peter	Holford, Esq;
Rofe	Fuller, Esq;	John Zephan.	Hollwell, Esq;
Alexander	Garden, M. D.	Edward	Hooper, Esq;
Mr. Francis	Geach.		



Donald           Monro, M. D.  
Edward          Montagu, Esq.  
Edw. Wortley Montagu, Esq;

GEORGE Duke of MONTAGU.

Charles        Moore, Esq;  
Robert         More, Esq;  
Thomas        Morell, D. D.  
John          Morgan, M. D.  
Corbyn        Morris, Esq;  
Michael       Morris, M. D.  
Leonard       Morfe, Esq;

Charles Morton, M.D. { *Ac. Imp. Nat.*  
                                  *Cur. & Pet. S.*

William       Mountaine, Esq;  
Samuel        Mufgrave, M. D.  
Sir William   Mulgrave, Bart.  
Robert        Mylne, Esq;  
Sir James     Naesmith, Bart.  
Mr Turberville Needham.

HENRY Duke of NEWCASTLE,

Peter         Newcome, A. M.  
Frank         Nicholls, M. D.  
John         Nicoll, Esq;  
John Mervin   Nooth, M. D.

HUGH D. of NORTHUMBERLAND,  
                                  *[Mus. Brit. Cur.]*

Christopher   Nugent, M. D.  
Sir Lucius     O'Brien, Bart.  
Rt. Hon. Robt. Ord.

CHA. DODGSON, L. Bp. of OSSORY.

Henry         Owen, M. D.

ROBERT LOWTH, L. Bp. of OXFORD.

General Pascal Paoli.

John          Paradise, Esq.  
John         Parker, Esq;  
William       Parker, D. D.  
Archibald     Patoun, Esq;  
Thomas        Pennant, Esq;  
Richard       Penneck, M. A.  
Thomas        Percival, M. D.  
William Philip Perrin, Esq;

John Lewis    Petit, M. D.  
James         Petty, Esq;  
Nathaniel     Pigott, Esq;  
William       Pitcairn, M. D.  
Mr. Joseph    Planta.

OTHER Earl of PLYMOUTH.

Allan         Pollok, M. D.

ALEXANDER Lord POLWARTH.

Edward        Pooie, Esq;  
W. HENRY Duke of PORTLAND,  
                                  *[Mus. Brit. Cur.]*

Mr. Percival Pott.

Will. Samuel Powell, D. D.

Thomas        Pownall, Esq;

Joseph        Priestley, LL. D.

Richard       Pulteney, M. D.

Mr. Henry     Putman.

WILLIAM Earl of RADNOR.

Matthew       Raper, Esq;

David         Ravaud, A. M.

Samuel        Reynardson, Esq.

Sir Joshua     Reynolds, Knt.

CHARLES Duke of RICHMOND.

Mr. David     Riz.

Matthew       Robinson, Esq;

Sir Thomas    Robinson, Bart.

CHARLES Marquis of ROCKINGHAM.

John         Roebuck, M. D.

ROBERT Lord ROMNEY.

Francis       Ruffell, Esq;

Thomas        Ryves, Esq;

Mr. Peter     Sainthill.

Count Jerom de Salis.

Henry Jerom de Salis, M. A.

Joseph        Salvador, Esq;

Erafinus      Saunders, D. D.

Sir George     Savile, Bart.

George        Scott, LL. D.

George Lewis Scott, Esq;

KENNETH Earl of SEAFORTH.

Mr. Samuel    Sharp.

John



Ds. Joh. Joseph.	Sue, <i>M. A. Parisiensis.</i>	Ds. — — de	Vigny, <i>Reg. Ac. Arch. Par. Sog.</i>
Ds. S. A. D. — —	Tissot, <i>M. D. Lausannensis.</i>	Ds. Anton. de	Ulloa, <i>Hispaniensis.</i>
Ds. Justus Joh.	Torkos, <i>M. D. Presburgensis.</i>	Ds. Franc. Arouët de	Voltaire.
Ds. Joseph. Ignat. de	Torres, <i>M. D. Hispan.</i>	Ds. Ricardus	Wall, <i>Reg. Hist. à Conf. &amp; Sec.</i>
Ds. Abrah.	Trembley, <i>Genevensis.</i>	Ds. Carolus	Walmesley.
Ds. — — Comes de	Tressan, <i>Reg. Sc. Acad. Par. Soc.</i>	Ds. Petr.	Wargentin, <i>Sec. R. S. A. Stockholm.</i>
Ds. Theod.	Tronchin, <i>M. D. &amp; Med. Prof. Gen.</i>	Ds. Joh. Hen. Georg.	Comes de Werthern, <i>Cub. Her. S. R. L.</i>
Ds. Fr. H. March. de	Turbilly, <i>Parisiensis.</i>	Ds. Joh. Hen.	Winkler, <i>Lipsiensis.</i>
Ds. Marfilius	Ventury, <i>Nob. Par. Reg. Hist. à Conf.</i>	Ds. Eustatius	Zanotti, <i>Astronom. Bononiens.</i>
Ds. Phillip	Venuti, <i>Nob. Cort. Lib. Port. Præp.</i>	Ds. Franc. Maria	Zanotti, <i>Sec. Ac. Sc. Inst. Bonon.</i>

A  
DISCOURSE  
ON THE  
*Attraction of Mountains,*

DELIVERED AT THE  
Anniversary Meeting of the ROYAL SOCIETY,  
November 30, 1775.

By Sir JOHN PRINGLE, Baronet,  
PRESIDENT.

PUBLISHED BY THEIR ORDER.

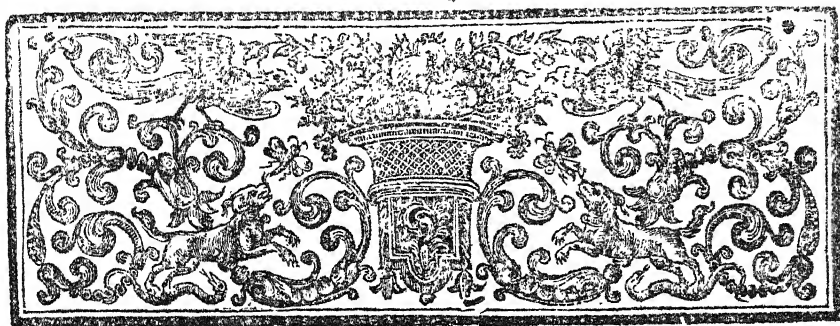


LONDON:  
Printed for the ROYAL SOCIETY.

---

M.DCC.LXXV.





GENTLEMEN,

THE satisfaction you discovered when a proposal was laid before you, *for measuring the attraction of mountains*, and the manner in which you received the account of what had been done to fulfil that view, were such indications of your applause, that your Council, ever attentive to your sentiments, have adjudged the prize-medal of this year to the Reverend *Nevil Maskelyne*, his Majesty's Astronomer at Greenwich, the author and conductor of that experiment. The many and valuable communications of our worthy brother, preceding this inquiry, you have never failed to distinguish; but these his late labours, undertaken at your request, with their successful result, related in his Paper, intitled *Observations made on the Mountain Schiehallien for finding its Attraction*, and inserted in the second part of the volume of your Transactions for this year, seemed to lay the

Society under such obligations, as your Council presumed you could not otherwise express than by the highest mark of your approbation. In consequence of this reflection, I have by their authority caused Mr. *Maskeelyne's* name, with the date of the present year, to be engraven on the medal, in order to perpetuate to him the honour you were this day to confer upon him ; if, after allowing me to recall to your remembrance some of the more interesting particulars of this disquisition and his operations, you should not refuse your sanction to the judgment of your Council.

I shall not consider the subject of attraction at large, nor touch upon any species of it, excepting what in latter times, by the effects, has been distinguished by the name of *gravity* or *gravitation* ; a property of bodies perceptible to the vulgar when things fall to the ground, but long acknowledged by this Society, to be a quality impressed by the Creator on all matter, whether of the earth or of the heavens, whether at rest or in motion : *He commanded, and it was created.*

The discovery of this extensive principle, the physics of astronomy, depended upon a just notion  
of

of the arrangement and motions of the spheres; for to understand their œconomy, it was necessary previously to know, which of the stars were quiescent, which moved, and in what manner. Whoever therefore found out the true celestial system, might be said to have paved the way to the knowledge of that sublime truth, the law by which the natural world is governed. But who were the inventors here? Were they Chaldeans or Ægyptians? Was it *Pythagoras*, or *Philolaus*, or any other Greek, either in their own country, or transplanted to the mathematical schools of Alexandria? I shall not enter upon that inquiry, as fruitless as obscure. All that is clear and to our purpose is, that some of the ancient Greeks conjectured rightly about the stability of the sun and the circular motion of the earth; but this was never a general persuasion, nor does it seem to have been mentioned any more after the age of *Ptolemy*, who in the second century did not so much invent a new system, as adopt that which now goes under his name, the prevailing one of his time, and nearly the same with that of *Aristotle*. This, though erroneous, was not perhaps incapable of improvements from celestial observations; but when the philosophy of the schools was united with the Ptolemaic hypothesis, and both were subjected

subjected to judicial astrology, then was astronomy debased to the level of the pretended learning of the dark ages that ensued, and increased their darkness.

But at the appointed time, when it pleased the supreme Dispenser of every good gift to restore light to a bewildered world, and more particularly to manifest his wisdom in the simplicity as well as in the grandeur of his works, he opened the glorious scene with the revival of a sound astronomy. *Copernicus* of Thorn (a Polish city in the Regal Prussia) endowed by nature with excellent talents, improved by a superior degree of mathematics, and by travelling, became early in life disgusted with the contradictions about the causes of the celestial phænomena. He had recourse, as he himself informs us \*, to every author upon the subject, to see whether any had been more consistent in explaining the irregular motions of the stars, than the mathematical schools ; but received no satisfaction, till first, from *Cicero*, he found that *Nicetas* had maintained the motion of the earth ; and next from *Plutarch*, that others of the ancients had been of the same opinion. *Cicero* had said, that *Nicetas the Syracusan* (according to Theo-

\* Præf. ad lib. de Revolutionibus Orbium Cœlestium.

phraftus) held that the heavens, the sun, the moon, the stars, in a word, the whole celestial bodies stood still, and that, excepting the earth, nothing moved in the world; but that whilst the earth with the greatest celerity turned round its axis, the same phenomena were produced, as if it stood still, and the heavens moved. And this some thought was also Plato's notion, but somewhat obscurely expressed\*.

Plutarch's words were, Others suppose the earth to be at rest; but Philolaus the Pythagorean, that it is carried in the ecliptic round the fire, like the sun and the moon. Heraclides of Pontus, and Ecphantus the Pythagorean, make the earth move like a wheel about its center from West to East, but not to change its place†.

From these quotations, and what Copernicus farther says‡, we find how little disposed that great man was to plume himself with the inventions of others; nay, rather anxious not only to do justice to those who had gone before him, but by their authority to screen himself from the censure of innovation, absurdity, and impiety, that awaited the publication of his doctrine. After all, the original genius of Copernicus was but little beholden for the discovery of those sublime truths, to

\* Cicer. Quæst. Academicæ. † Placit. Philos. lib. 3. cap. 3. ‡ Ibid.



either *Nicetas* or *Plato*, since it appears from *Cicero* that these two believed both the moon and the planets to be motionless. Nor could he be more assisted by *Philolaus*, who taught that *the earth turned round a fire*; but this fire could not be the sun, because that ancient compares the motion of the earth about *the fire*, to the revolution of the sun and moon about the earth. Lastly, what little light *Copernicus* could draw from *Heraclides* and *Ecpbantus*, I scarcely need say, since they, though admitting the diurnal motion of the earth, denied the annual.

But if *Copernicus* sought to do justice, why did he not rather cite a clear and express passage in the *Arenarius* of *Archimedes* for the fixed state of the sun, and for the motion of the earth in a circle round his body? *What most philosophers call the world* (says that famous ancient) *is a sphere, of which the center is that of the earth, and whereof the semi-diameter is equal to a right line joining the centers of the earth and the sun.* But *Aristarchus the Samian*, refuting this opinion, has advanced an hypothesis, whereby the world should be many times greater than what is here said; for he supposes that the fixed stars and the sun remain immoveable, and that the earth is

2

carried

*carried in a circle round the sun, placed in the middle of it's course* \*.

Thus far *Archimedes*, who seems not to disapprove the system, but who explains it no farther, as what he had quoted was sufficient for his purpose. It is probable that the penetrating genius of *Aristarchus* had discovered the true arrangement of the whole celestial bodies, and thereby totally anticipated *Copernicus*; but that circumstance is no where, that I know of, recorded; and otherwise, we should acquit our illustrious reformer of plagiarism with regard to *Aristarchus*, since neither the *Arenarius* of *Archimedes*, where that passage is found, nor indeed any other of his valuable remains, had seen the light before the death of *Copernicus*. This extraordinary person had even before the meridian of life completed his discoveries, and comprised them in his book *de Revolutionibus Orbium Cœlestium*, his only work; but which he had prudently suppressed, till he had maturely considered his subject, and had found a necessary and powerful patron, the pope himself, *Paul III*, a lover of astronomy, to protect him. Alluding to the admonition of the poet, he tells the Pontif, *he had*

\* *Archimed. Arenar. ed. Oxon. 1676.*

*suffered that fruit of his labours to ripen, not nine years only, but four times nine* \*. Consenting at last to the publication, he committed the care of the impression to some friends in a distant city, from whom he received the finished copy a few hours before he expired †.

Few compositions have destroyed more riveted errors, or established more important truths. Here, instead of an absolute state of rest for the earth, it's triple motion is ascertained, the diurnal about it's axis, the annual about the sun, and that other known by the term *precession of the equinoxes*; all which till then had been referred to the motion of the heavens. He likewise demonstrated the double orbit of the moon, that is, her menstrual motion about the earth, and her annual about the sun. Nor did the wise *Copernicus* stop here; for, after laying this solid foundation of the celestial physics, he began the superstructure, by surmising a principle of *attraction* to be inherent in all matter. Thus, in refuting the peripatetic notion, that bodies fall to the ground, because by a law of nature every thing heavy tends to the center of the universe (which they supposed to be in the center of the earth) he observed, that *the earth could not be the center of the orbits of several of the*

\* Præfat. ad lib. de Revolut. † Gassend. in vita Copernic.

planets, because of the apparent irregularities of their motions, and therefore could not be the center of the universe : hence, according to these philosophers, there must be more centers than one ; and if so, who could tell the true center, toward which all bodies were to gravitate ? As for gravity, says he, I consider it as nothing more than a certain natural appetite (appetentia) that the Creator has impressed upon all the parts of matter, in order to their uniting and coalescing into a globular form, for their better preservation ; and it is credible that the same power is also inherent in the sun and moon and planets, that those bodies likewise may constantly retain that round figure in which we behold them \*. Farther, Copernicus looked upon the sun as the chief governing power of the earth and all the other planets ; for after placing the great luminary in the center, he cries out with rapture, *Profecto tanquam in solio regali sol residens circumagentem gubernat astrorum familiam* †. Nor was this government understood to be exercised by any other power than that of attraction ; as may be inferred from some of the last words of the celebrated *Tycho Brahe*, who perceiving the approach of death, called for the famous *Kepler* (then a young man, and his assistant in his observatory

\* De Revolut. Orb. Cœlest. lib. 1. cap. 9. † Ibid. cap. 10.

at Prague) and after charging him with completing and publishing the astronomical tables which he was leaving unfinished, thus addressed him: *My friend, although what I ascribe to a voluntary, and as it were an obsequious motion of the planets round the sun, you attribute to an attractive energy of that body; yet I must entreat you, that in the publication of my observations, you would explain all the celestial motions by my hypothesis, rather than by that of Copernicus, which I know you would otherwise incline to follow* \*.

From this passage, which I have taken from the life of *Tycho Brahe*, it would seem, that though that other excellent astronomer was not insensible of some influencing power of the sun over the planets, he would not however express it by so strong a term as *attraction*. But in what manner *Kepler* complied with the request of his dying patron, it is not to our present purpose to mention, and therefore we shall only observe, that in his own works he constantly maintains the doctrine of attraction, and carries it even farther than ever *Copernicus* had done. Thus he calls gravity *a corporeal and mutual affection between similar bodies, in order to their union* †. Again he remarks with *Copernicus*, against the peripatetics, that

\* Gassend. in Vit. Tych. Brahe, cap. 5. † Astron. Nov. in Introduct.

heavy bodies do not tend to the center of the universe, but to the center of those larger round bodies of which they make a part; so that if the earth were not spherical, things would not fall from all points towards its center. If a stone were to be placed at a distance from another stone, in any part of the universe, without the sphere of action of a third body, like two magnets, they would come together in some intermediate point, each advancing, in space, in the inverse proportion of their quantities of matter. Hence if the moon and the earth were not by some power kept asunder in their respective orbits, they would move towards one another; the moon making 53 parts of the way while the earth made one, supposing their densities equal\*.

From the same principle *Kepler* accounted for the general motion of the tides, to wit, by the attraction of the moon, and expressly calls it *virtus tractoria quæ in luna est*†. He adds, that if the earth did not exert an attractive power over its own waters, they would rise and rush to the moon‡. Farther, we find him suspecting certain irregularities in the motion

\* Astron. Nov. in Introduct.

† Ibid.

‡ Ibid.

of the moon to be owing to the combined action of the earth and sun upon it's body\*. These, and other reflections concerning the universality of attraction, he accompanies with an ingenious anticipation of a law of nature, from conjecture only, but which was afterwards made out by experiments. The schools had taught, that *some bodies were by their nature heavy, and so fell to the ground, and that others were by their nature light, and therefore mounted upwards*: but *Kepler pronounced, that no bodies whatsoever were absolutely light, but only relatively so; and consequently, that all matter was subjected to the law of gravitation* †.

Hitherto the genius of *Kepler* had been fortunate, in tracing out that great principle which hindered the planets from flying off from the sun: but what kept them from falling into that mass of fire, and what power perpetuated their motion in their orbits? Here his sagacity failed him, and left his imagination to furnish the idea of a system of *vortices* for *Descartes*.

But howsoever incomplete these notions were concerning gravitation, yet in justice to their distinguished

\* Astron. Nov. cap. 37.

† Ibid. in Introduct.

authors,

authors, *Copernicus* and *Kepler*, I thought proper to commemorate them on this occasion, as none before them had expressed themselves so fully, and with so much truth on that curious subject; and as none, from their days, to those of Dr. *Hooke*, made any such improvement, as would apologize for my taking up so much more of your time in recalling their sentiments to your remembrance. Let it suffice to mention, that the first who in this country embraced that doctrine was Dr. *Gilbert* \*; but who did not properly distinguish between attraction and magnetism; and that the next was lord *Bacon*, who, though not a convert to the *Copernican* system, yet acknowledged an attractive power in matter †. In France we find *Fermat* and *Roberval*, mathematicians of great eminence, of the same opinion ‡; and in Italy *Borelli*, after *Galileo* ||, who was the first in that country who conceived that idea, but far from that precision and extension we find it in his contemporaries *Bacon* and *Kepler*.

Before we pass from *Kepler*, it will be proper to observe, that this great improver of astronomy did not, perhaps,

\* De Magnete. † Nov. Organ. lib. 2. aphor. 36, 45, 48. Sylv. Sylvar. cent. 1. exp. 33. ‡ Montucla Hist. des Mathem. part. 4. liv. 8. || Syst. Cosmic.



after all, contribute so much to the advancement of this theory, by those conjectures which I have related, as by some astronomical deductions from *Tycho Brabe's* observations, since known by the name of *Kepler's Laws*. The first was, that the planets move not in circular, but in elliptical orbits, of a small eccentricity, whereof the center of the sun makes one of it's *foci*. The second, that the same planet describes about the sun equal areas in equal times. The third, that in different planets, the squares of the periodic times are as the cubes of their mean distances from the sun.

Such were the preparatives to the true philosophy, and indeed excellent materials for the architect then unborn. But till sir *Isaac Newton* appeared, notwithstanding the numerous and momentous discoveries that had been made in the heavens, by *Copernicus*, *Tycho Brabe*, *Galileo*, *Kepler*, and others, yet astronomy, as lord *Bacon* complained, still remained but a mathematical study. The passage to which I allude is long, but, as tending to illustrate more than one particular relating to my subject, I cannot forbear trespassing on your indulgence by the citation. *Although astronomy (says Bacon) has not been founded amiss upon observation of the phænomena, yet the superstructure has hitherto kept low and weakly.*

*In truth that science presents to the human understanding such an object as Prometheus did of old to Jupiter, when, meaning to impose upon that deity, he offered upon his altar, instead of a live victim, the hide of a large bullock, stuffed with straw, leaves and osier branches. In like manner astronomy exhibits the externals of the celestial bodies, as the cuticular part of heaven, fair indeed and artificially formed into a system; but the entrails and the fountains of life are wanting, that is, the physical causes and reasons; from which and from astronomical hypotheses, a theory should be drawn, not adequate only to account for all the phenomena; but for the substance, the motion, and influx of the heavens as they are in nature.... Scarcely is there one to be found who has inquired into the natural causes, either of the substance of celestial matter, or into the reason of the swiftness or slowness of the heavenly bodies, acting upon one another; or into the various degrees of motion of the same planet, or into the motion from East to West, or of the contrary direction; nor into the progressions, stations, and retrogradations of those bodies.... Nor into the causes of the apogæum and perigæum.... I say, inquiries of this kind have scarcely been attempted, nor indeed any labour bestowed upon the subject, excepting in the way of mathematical observations and demonstrations. So that*

C

*astronomy,*

*astronomy, such as it now is, can only be reckoned among the mathematical arts; not without considerable diminution of its dignity; since were it to maintain its rights, it might rank itself as the noblest branch of philosophy. For he that shall reject the fictitious divorces between the superlunary and sublunary bodies, and shall duly attend to the appetences and most general affections of matter (which both in the earth and in the heavens are exceedingly powerful, and indeed pervade the universe) will receive from what he sees passing on the earth clear information concerning the nature of celestial bodies: and contrariwise, from motions which he shall discover in the heavens, will learn many particulars relating to the things below, that now lie concealed from us. Wherefore the physical part of astronomy we mark as wanting, and call it the astronomia viva, the animated astronomy, in opposition to the stuffed bullock of Prometheus\*.*

The great desideratum was supplied, and from the bosom of this Society, in the publication of the *Principia*, the immortal work of Newton. There the illustrious author evinces truths that had been only surmised before; and after establishing by a just analysis the laws

of attraction, in a synthetical method proceeds to explain by them the motions and appearances of the heavenly bodies. Had not *Newton* lived, *Bacon* might have passed for a visionary speculator; but since the demands of that noble author upon the human intellects have been so fully answered in the productions of sir *Isaac Newton*, shall we not revere those powers of his own mind, that could, in that dawn of philosophy in which he lived, so well descry what parts were wanting, and what were the means of attaining them?

*Newton* in a posthumous treatise, *de Systemate Mundi*, composed before the publication of the *Principia*, and mentioned there, has said, that *some of the latter philosophers had sought to account for the course of the planets in their orbits by the action of certain vortices, as Kepler and Descartes; or by some other principle of impulse or attraction, as Borelli, Hooke, and others of our nation.* From this passage it would seem, that in those times there had been more conjectures formed concerning attraction, than what were published; for excepting *Gilbert*, who vainly attempted to explain the mundane system by magnetism, and lord *Bacon*, who never acceded to the *Copernican* hypothesis\*, I have found none of our na-

\* Atque harum suppositionum absurditas, in motum terræ diurnum (*quod nobis constat falsissimum esse*) homines impegit. Bæc. de Dign. & Augm. Scient. lib. 3. cap. 4.

tion, *Hooke* excepted, who in this way have left any thing on record worthy of your notice. He indeed, the early, the ingenious and most useful member of this Society, advanced in this research far beyond all that had gone before him. But I shall not enlarge upon his improvements, as you have in your hands his *Cutlerian Lectures*, which contain them, and as I have already but too long dwelt on this part of my subject. It will ever redound to the praise of *Hooke*, that *Newton* has associated him with himself in maintaining the true regulating cause of the course of the planets\*. As to *Borelli*, though I have found in one of the pieces (a scarce one) of that learned Italian, a passage that certainly favours attraction; yet as it is neither so full, nor so explicit upon that point, as several others which I have cited, I must suspect that those parts which *Sir Isaac* had in his eye have escaped my observation†.

\* M. Montucla has done great justice to Dr. *Hooke*, in this and other particulars, in his excellent work, *Hist. de Mathem. part. 4. liv. 8.*

† This is the passage alluded to: *Præterea manifestum est, quemlibet sive primarium, sive secundarium planetam aliquem insignem mundi globum quasi virtutis fontem circumdare, qui ita eos stringit, atque conglutinat, ut ab ipso nullo pacto abstrahi possint; sed ipsum, quacunque contendente, perpetuis continuisque orbibus cogantur consequi: videmus enim Saturnum, Jovem, Martem, Venerem, atque Mercurium Solem ipsum, Medicæa Sidera Jovem, Hugenianumque Sidus Saturnum circumire, non secus, ac circa Telluris Globum Luna ipsa revolvitur. Joa. Alph. Borelli Theor. Medic. Planetar. ex causis Physicis deductæ, lib. 1. cap. 2. p. 5. Florent. 1666. 4º.*

The great completer of the doctrine of universal gravitation had the satisfaction to find, from the reception it met with in this Society, that he had not laboured in vain ; nay, perhaps no philosophical author was ever more admired and followed in his own time, and in his own country, than *Newton* was in these kingdoms. With regard to others, *we are not to wonder*, as remarked by his eloquent Eulogist, *if philosophers upon the first publication of the Principia took the alarm at the term attraction, as fearing the return of the occult qualities ; or if, considering the difficulty of the subject, and the few words employed in explaining it, they wanted time fully to comprehend it* \*. These obstacles have been removing by degrees, and the way at last has been so effectually cleared, that the name of *Newton* is not perhaps held in more estimation here, nor his principles more cordially embraced, than in those very societies of the learned, abroad, which at first shewed most unbelief, and at whose conversion therefore we ought most to rejoice.

The Royal Academy of Sciences, whilst in an uncertain state between the old and new system of philosophy,

\* Eloge de *Newton*, par M. de Fontenelle.

having, for one of the decisive experiments, measured some degrees of latitude upon an arch of a meridian passing through Paris, and compared this mensuration with others; inferred the earth to be a spheroid with the longest diameter passing through its poles; but sensible that this operation had not been so unexceptionably conducted, as to satisfy either the followers of *Newton* or those of *Huygens*, who both required a spheroid flattened at the poles, resolved upon a farther and more accurate trial. With this view, in the year 1735, some chosen members from that illustrious Body were sent to the polar circle, and others to the equator; at which places the differences of degrees being greater, the point in dispute might be determined with less danger of error. How much to the honour of *Newton* and *Huygens* the result was, is sufficiently known. All that is necessary to be mentioned here, is that in the year 1738, whilst the academicians were still in Peru, it occurred to *M. Bouguer*, one of that number, to put the *Newtonian* system to another test, by inquiring into the attraction of mountains. This idea, which was originally from *Newton* himself, *M. Bouguer* communicated to his colleague *M. de la Condamine*, who readily assisted in making the trial\*. Those gentlemen

\* *Bouguer*, Figure de la Terre, sect. 7. *De la Condamine*, Journal du Voyage à l'Equateur.

were persuaded, that if the whole mass of the earth were really possessed of such a property, a high mountain, such as nature had abundantly provided in that country, would shew some proportionable degree of it. That the largest of the *Andes* was indeed but a small object in comparison of the earth; nevertheless they reckoned, by a rough computation, that the attraction of *Chimborazo*, which they deemed the best for their purpose, would be equal to about the 2000th part of the attraction of the whole earth. Now, here the mountain acting as one, whilst the earth as 2000, the direction of gravity would be visibly turned out of the vertical line, for as much as this direction would be 1' and 43" towards the mountain. But how was this deflexion to be estimated? Only by finding the quantity of deviation of the plumb-line from a vertical position, by means of stars. In order to attain this point, they found it most convenient in their present circumstances to take the distance of several stars from the zenith at two stations, one on the south side of *Chimborazo*, and the other a league and a half to the west; that is, at such a distance from the first station, as that the plumb-line should be but little affected by the mountain. This disposition being made, they proceeded



to their operations, of which we have a full and clear account by M. *Bouguer*, in his valuable treatise intituled *Figure de la Terre* ; but of M. *de la Condamine*, we have only a short abstract of the narrative he presented to the Academy ; which abstract is contained in his curious *Journal of his Voyage to the Equator*.

From both it appears, that though those learned persons, during the time employed in this experiment (which the inclemency of the air, at that height in the atmosphere, forced them to make very short) I say, though during this time they spared no pains, yet their observations not only varied from one another, but seemed to be little satisfactory to themselves. M. *Bouguer* says, that instead of  $1' 43''$ , which the plumb-line ought to have declined from the true vertical line, the total declension amounted only to seven seconds and a half : an effect that fell far short of the expectations of a *Newtonian*. But those candid gentlemen take notice, that, *as on one hand we are ignorant of the density of the internal parts of the earth, which may be considerably greater than what appears by its surface ; so, on the other, Chimborazo, which they believed likely to be as solid as any other parts of the surface of the earth, might nevertheless*

*in many places be hollow. Nay, M. de la Condamine tells us, that he was afterwards informed of a tradition in the country, that this very mountain had once been a volcano; and adds, that whilst he and his colleague were about their experiment, they had actually found some calcined stones upon it. From which circumstances he infers, that if one cannot just draw from this trial an absolute proof of the Newtonian attraction, one can far less form any conclusion against it. M. Bouguer goes farther and observes, that if we will be satisfied with the bare fact, it is certain from this experiment, that mountains do act at a distance, but that their action is much less than what might be expected from their bulk. He concludes his account in the true spirit of a philosopher, by saying, that as in France, or in England, a hill may be found of a sufficient height for the purpose, and especially if the observer would double the action, by making a station on each side; he should be happy to hear, on his return to Europe, that the experiment had been repeated, whether the result tended to confirm his observations, or to throw some better light upon that inquiry. If the Society have fulfilled the views of that worthy man, who thus called upon them, we have to regret that he did not live long enough to share the satisfaction with us.*

I come now to Mr. *Maskeelyne's* labours, upon which I shall not expatiate, as I have already taken up too much of your time, and as I judge it unnecessary to dwell long upon that part of my subject, which you have so lately heard in his own words, and which you will have in a few days published at large in your Transactions.

I need only remind you, that the zenith distance of a star on the meridian being observed at two stations under the same meridian, one on the south side of a mountain, the other on the north; if the plumb-line of the instrument be attracted by the mountain out of its vertical position, the star will appear too much to the north by the observation at the southern station, and too much to the south by that at the northern station; and consequently the difference of the latitudes of the two stations will be found by these observations greater than it really is. And if the true difference of their latitudes be determined by measuring the distance between the two stations on the ground, the excess of the difference found by the observations of the star above that found by this measurement, must have been produced by the attraction of the mountain, and it's half will be the effect of such attraction on the plumb-line at each observation, supposing the mountain attracts equally on both sides.

To perform this experiment, Mr. *Maskelyne* made choice of the Mountain *Schehallien* in Perthshire, in North Britain, of which the direction in length is nearly east and west, it's height above the surrounding valley at a medium is about 2000 feet, and it's highest part above the level of the sea is 3550 feet. As the greatest attraction of the mountain was to be expected about half way up it's sides (which happened conveniently for the purpose of the experiment to be pretty steep) two stations for an observatory were accordingly chosen, one on the north, and the other on the south side of *Schehallien*. The instrument with which he observed the stars was an excellent sector made by Mr. *Sisson*; and Mr. *Maskelyne* has related at large all the precautions he took both for adjusting this instrument in the meridian at each station, and for satisfying himself that the line of collimation remained unaltered. From observations of ten stars near the zenith, he found the apparent difference of the latitudes of the two stations to be  $54''.6$ ; and from a measurement by triangles, formed from two bases on different sides of the mountain, he found the distance of their parallels to be 4364 feet, which, in the latitude of *Schehallien*, viz.  $56^{\circ} 40'$ , answer to an arch of the meridian of  $43''$ : this is  $1''.6$  less than that found by

the sector. It's half therefore  $5''$ , 8 is the mean effect of the attraction of the mountain: and from it's magnitude, compared with the bulk of the whole earth, Mr. *Maskeelyne* discovered the mean density of the earth to be about double that of the mountain.

In the execution of this interesting experiment, our worthy brother has not only exerted a patience and perseverance, but a sagacity and judgment which must ever redound to his honour. All doubts about an universal attraction must at last be terminated, and every philosopher in that respect must now become a *Newtonian*.

If I have related but two experiments that have been made, the first by the *French* academicians, and the other by Mr. *Maskeelyne*, it is because no more have come to our knowledge; nor do I believe that more have actually been executed. For if, in occasional measurements of degrees of the meridian in different parts of Europe, those employed have found varieties arise in their measures that they could not otherwise account for, than from the attraction of the mountains among which they carried on their operations, and accordingly

have referred those irregularities to that very cause; such conjectures we admit may be well founded, but the measurements whence they arise we cannot reckon among the experiments we now treat of.

But was not the doctrine of an universal attraction so fully demonstrated by *Newton*, as not to require any farther proofs from experiments? Demonstrated it was, but not to the conviction of every individual. True Philosophy condescends to adapt her instructions to different capacities, and is as willing to inform by palpable experiments as by geometrical demonstrations. But to say the truth, something seemed wanting here for the satisfaction of even the more enlightened minds. Such we reckon those were who first made the trial. And did not *Huygens* himself, one of the greatest philosophers and geometricians of his age, find difficulties about this principle, even after the publication of *Newton's Principia*? nor do we learn that the doubts of that great man were ever removed\*. To say nothing of the celebrated *Leibnitz*, and his numerous followers, who to this day are either wholly unbelievers in attraction, or at best but sceptics on that article.

\* Vid. *Hugen. Dissert. de Caus. Gravitat.*

You have therefore, Gentlemen, the satisfaction to think that you have completed a great and acceptable work to the scientific world ; and that, though this has been a costly experiment, your gracious PATRON, who so liberally furnished the means, will highly approve your expending his benefaction so much for the advancement of Natural Knowledge and for the benefit of the public ; and will so much the more be disposed to shew you the like favour on future occasions.

BUT for those who wanted no fresh proofs of the universality of attraction, they must still partake of the advantages accruing from this experiment, as being not only the first that has been made, but the best that could be devised, for estimating the mean density of the earth. The operation in Peru was too imperfect for that purpose, and had the circumstances of that trial been more favourable, yet the suspicion of their mountain having been once a volcano, was a sufficient reason for admitting no evidence from it in this part of our inquiry. But for Schehallien, as it's appearance was particularly rocky, and as several specimens of those rocks have been presented to the Society, and acknow-

7

ledged

ledged to be mineral substances that had never passed through fire, we may consider that mountain as one of the proper patterns of the density of the surface of the earth.

These, Gentlemen, are the fruits of the operations of Mr. *Maskekyne*, during a residence of four months in a mean hut, on the side of a bleak mountain, and in a climate little favourable to celestial observations. To these inconveniencies, however, he submitted with patience and complacency, as he went at your request and in pursuit of science. You have heard his chief conclusions; but permit me to add, that as this is a new mine opened in the field of nature, I am confident that these will not be the only productions; but that, as in all great and successful experiments, there will be in the prosecution of this subject some valuable truths brought to light, of which at present we can form no particular conjecture. Mean while we have the pleasure to find the doctrine of *universal gravitation* so firmly established by this finishing step of analysis, that the most scrupulous now can no longer hesitate to embrace a principle that gives life to Astronomy, by accounting for the various motions and appearances of the Hosts of Heaven.



---

MR. MASKELYNE,

THE judgment, Sir, of the Council, in awarding you the prize, having received the sanction of the ROYAL SOCIETY, I do, in the name and by the authority of that illustrious Body, present you, their most worthy Brother, with this sincere pledge of their affection; as the lasting token of their acknowledgment for your several ingenious and useful communications, and more particularly for this last painful and capital experiment, which adds no small lustre to their Transactions. And after expressing their grateful sentiments for what you have already done for their service, I would farther say, that they persuade themselves, from your talents, your love of your profession, and your happy period of life, you will continue steadily to pursue that path which you have so early entered upon, and which so surely leads to great and useful discoveries. You have, Sir, in charge the noblest branch of Natural Philosophy: such it has ever been held by this Society, and as such it ever has been cherished and cultivated by them. And they flatter themselves that their cares and sollicitude have not been fruitless

fruitless ; since, from their first institution to this day, there have never been wanting some excellent men in that line, to promote the science, and do honour to this Community. But so transcendently great is that part of the creation, that though the Divine Author has vouchsafed, in these latter days, to open to the humble and patient inquirers into Nature the *Causes of Things*; yet we must still cry with the ancient sage, *Lo, these are part of His ways, but how little a portion is heard of them !* As much then remains to be explored in the celestial regions, you are encouraged, Sir, by what has been already attained, to persevere in these hallowed labours, from which have been derived the greatest improvements in the most useful arts, and the loudest declarations of the power, the wisdom, and the goodness of the Supreme Architect in the spacious and beautiful fabric of the World.

F I N I S.



